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ABSTRACT

Without continued technical innovation and rapid conversion of these innovations to quality products and processes, America cannot remain a viable competitor in today's global markets. This volume contains five presentations at a symposium convened by the National Academy of Sciences, the National Academy of Engineering, the Institute of Medicine, and the National Research Council. Participants were research and development (R&D) managers and scholars from industry and government. Chapters contained in this document are: (1) "U.S. Economic Policy: Implications for R&D" (Nathan Rosenberg); (2) "Reduction to Practice: The Development and Manufacturing Cycle" (Ralph E. Gomory); (3) "The Role of Management in R&D Strategy: A Case Study" (Richard J. Mahoney); (4) "Human Resources for Industrial Research and Development" (Erich Bloch); and (5) "Cooperative R&D Ventures: Collaboration in the Semiconductor Industry" (William F. Brinkman). Discussions pertaining to each presentation are also included. (YP)

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INDUSTRIAL R&D AND U.S. TECHNOLOGICAL LEADERSHIP

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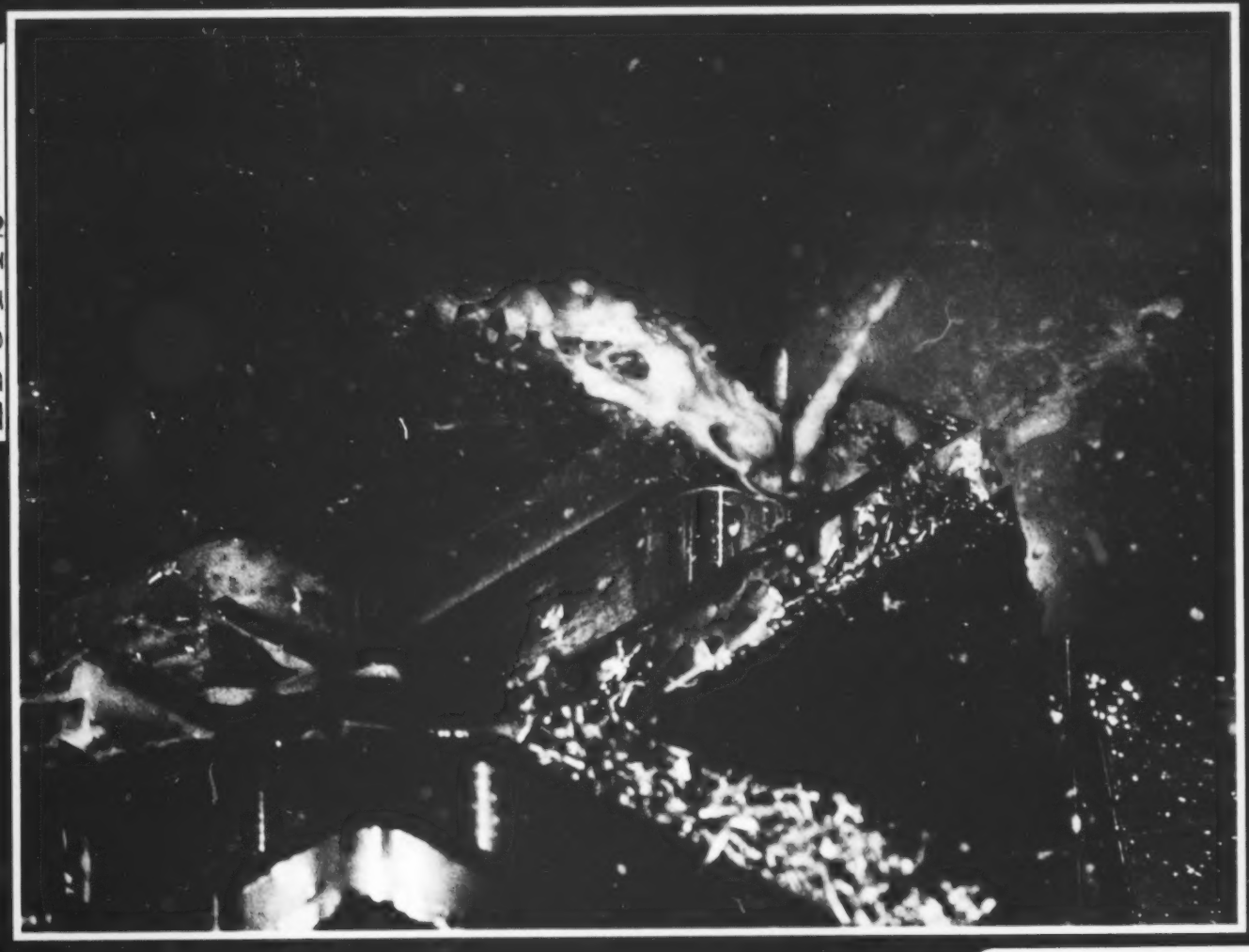

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INDUSTRIAL R&D AND U.S. TECHNOLOGICAL LEADERSHIP

From the Proceedings of a Symposium

National Academy of Sciences
National Academy of Engineering
Institute of Medicine
National Research Council

NATIONAL ACADEMY PRESS
Washington, D.C. 1988

The National Academy of Sciences is a private, nonprofit, self-perpetuating society of distinguished scholars engaged in scientific and engineering research, dedicated to the furtherance of science and technology and to their use for the general welfare. Upon the authority of the charter granted to it by the Congress in 1863, the Academy has a mandate that requires it to advise the federal government on scientific and technical matters. Dr. Frank Press is president of the National Academy of Sciences.

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PREFACE

A commitment to industrial research and development in all its forms—basic research, applied research, and product development—is crucial to the nation's continued economic vitality. Few in business, government, or academe would challenge this assertion. The long-term benefits of R&D—new products and markets—provide the basic rationale for this view. In addition, the steady erosion of America's dominance of world markets in recent years in the face of Asian and European competition has highlighted the importance of a robust national program of industrial R&D. Without continued technical innovation and rapid conversion of these innovations to quality products and processes, the nation cannot remain a viable competitor in today's global markets.

Yet obstacles to the smooth functioning of the R&D enterprise are many. Economic uncertainty can lead to large annual fluctuations in R&D budgets—anathema to any long-term program of research. The complexity and expense of research activities continue to increase. The rapid transfer of new technologies, both between competing companies and between competing nations, shortens the time available to recoup the costs of R&D investment. Because of such factors, the growth rate of R&D funded by industry has slowed, even though R&D expenditures by U.S. industries continue to increase overall.

Funding for R&D activities in the United States by industry, government, and universities will total an estimated $132 billion in 1988, with a work force of over 800,000 scientists and engineers. Of this total, government and industry will contribute approximately equal shares—an estimated $65 billion and $63 billion, respectively. Nearly half of the R&D funded by the government, however, is conducted in industrial labs—meaning that about three-quarters of all R&D in the United States is performed by industry, with the remaining work divided among university, government, and other nonprofit labs. Industrial laboratories therefore play a central role in the nation's R&D effort, not only in product development but in applied research and basic research as well.

In absolute terms, the United States spends more than any other nation on research and development, but several competitor nations devote a larger share of their gross national products to R&D. While the United States invests less than 2 percent of its GNP on nondefense R&D, Japan invests nearly 3 percent of its GNP in nondefense R&D, West Germany over 2.5 percent, and France about 2 percent.

Of course, successful industrial R&D does not depend on funding alone, nor does success in the lab necessarily mean success in the marketplace. The industrial policy of the government, the philosophy of corporate management, and, most of all, the ability of a company to integrate its research activities into its development and manufacturing cycle are also important factors in the R&D equation.

On November 10, 1987, a group of R&D managers and scholars from industry and government met in Washington, D.C., to address these issues at

the Symposium on Industrial R&D Strategies and U.S. Technological Leadership. Sponsored by the National Academy of Sciences, the National Academy of Engineering, and the Institute of Medicine, the symposium focused on the challenge facing the United States in maintaining its scientific and technical capabilities and in wielding these capabilities to compete effectively in the world economy. This volume contains the presentations of five leading figures in the R&D enterprise who spoke at the symposium, along with questions and answers that followed their talks and commentaries given by other symposium participants. By offering the fruits of the symposium to a wider audience, it is hoped that this book will stimulate an awareness of the nation's critical need to nurture its research endeavors or face waning industrial competitiveness.

As chairman of the symposium, I would like to extend the appreciation of the speakers and participants to Anne Keatley, Executive Director of the National Research Council's Office of Government and Public Affairs, who developed the symposium program; to Lois Edwards, Staff Officer of the Academy Industry Program, who organized and coordinated the symposium; to Gregory Mock, who edited this volume; and to Pamela Steele Reznick, who designed it.

David Packard
Chairman of the Board
Hewlett-Packard Company

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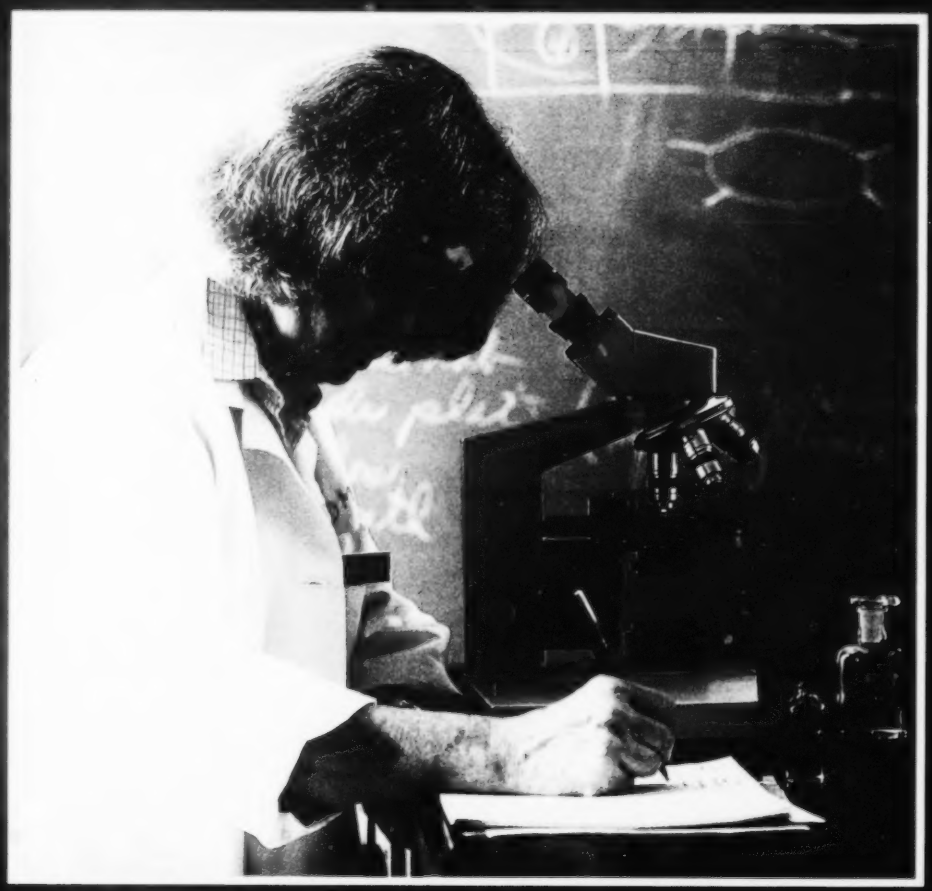


U.S. ECONOMIC POLICY: IMPLICATIONS FOR R&D

NATHAN ROSENBERG
Professor of Economics, Stanford University

America's future success as an industrial nation in an increasingly competitive world depends heavily on the ability of firms to formulate and to execute effective research and development strategies. I am sure this statement will command almost universal agreement, even among people who simultaneously support policies that virtually assure the failure of most sensible R&D strategies.

Such an assertion may seem paradoxical; it isn't. The problem is that many people, including some who take great personal pride in being well informed, do not really understand what R&D activities are all about. Nor, perhaps more importantly, do they appreciate what are the larger economy-wide forces conducive to the successful outcome of these R&D activities.

The phrase "research and development" conjures up images of white-coated scientists performing experiments to unlock nature's most tightly guarded secrets. Of course, research and development does include all such activities. But if we consider total R&D expenditures in the United States today, regardless of the sources of finance, it turns out that R&D activities are overwhelmingly "D" and not "R."

In recent years, about 68 percent of all research and development has been development. Less than a quarter—in fact, about 21 percent—has been applied research, and only about 12 percent has been basic research. Thus, most of the research and development enterprise occurs downstream from the work of the basic scientist, and the great bulk of all R&D activity is performed in private industry.

So, the public needs to understand far better than it presently does what the "D" of "R&D" is all about. It needs a better appreciation for the very high level of creativity that is involved in converting a new scientific insight into a commercially successful product. And it needs a much clearer understanding of the kind of economic environment that is conducive to such commercial success.

In fact, we know that a great scientific breakthrough is usually very remote from useful applications of that new knowledge, if, indeed, applications ever occur. There is currently great excitement about recent discoveries in the area of superconductivity. Clearly, such excitement is well justified in terms of what these breakthroughs may augur for the future. But I suspect that it may be many years—probably decades—before that scientific progress can be translated into magnetically levitated trains, the transmission of electricity without loss, or the storage of electricity.

Designing products that exploit new knowledge about high-temperature superconductors and then designing and building the technology that can produce these new products are all extraordinarily difficult, time-consuming, and expensive activities. Thus, it is essential to understand the complexities of the development process.

We must realize that designing novel products, testing them, building prototypes, testing them again, experimenting with pilot plants, inventing new manufacturing processes, making the innumerable minor modifications to reduce costs slightly here or improve performance slightly there—these are much less exciting activities than "breakthrough science," especially when looked at from an academic perspective. They are activities that do not win Nobel Prizes. But they are, in large measure, what technological progress is really all about.

Let us now turn to some aspects of the present-day scene. New technologies today are being transferred across national boundaries more rapidly than a few decades ago. There are a number of reasons for this. The main reason has to do with the specific characteristics of some of the new technologies available since World War II: new techniques of transportation and communication, such as the jet airplane, telecommunication, and the computer.

This ease of transport and communication effectively guarantees that, no matter what country

may have originated the new technology, the innovation will soon be made available to other countries. The specific details of a major new product design, for example, can be transmitted instantly to any part of the world. In addition, increasingly sophisticated computer-aided manufacturing systems are making it possible to rapidly transfer much of the required manufacturing capabilities of a widening range of products.

Moreover, the multinational firm and its growing importance on a global scale is an excellent organizational device for carrying out these technology transfers. In fact, American multinational corporations are not just conducting more of their manufacturing operations abroad. They are also conducting an increasing proportion of their R&D activities abroad for a variety of reasons, including relative costs and the nature of government regulations. A good example of this is the pharmaceutical industry. It is symptomatic of the degree of internationalization of the research process that the Nobel Prize in physics in 1986 was won by two German scientists working in Switzerland for an American firm—IBM.

Finally, the great increase in development costs that are associated with new technologies in such sectors as aircraft and telecommunications has led to a systematic search for overseas partners in joint venture arrangements. These joint ventures offer the prospect of a sharing of high development costs, as well as other possible commercial benefits. But they also assure even speedier international diffusion of new technologies. This more rapid transfer has an important economic implication. That is, it reduces the purely economic advantages in high-technology markets that might once have accrued to American firms because of America's splendid scientific research capabilities.

These capabilities have, in the past, generated economic advantages and new technological developments that flowed out of research at the scientific frontier. I am not denying the great economic benefits that continue to flow from the conduct of scientific research. Rather, I am denying that those benefits necessarily flow in the form of competitive economic advantages to the country conducting the research. This is an important distinction.

Great Britain's economic experience in the years since the Second World War is a case in point. The British experience convincingly demonstrates the insufficiency of high-quality science when it is not associated with the complementary managerial and engineering skills, and when the economic environment fails to offer high rewards to technological innovators or to the adopters of newly available technologies.

Great Britain for many years has received more Nobel Prizes in science than America on a per capita basis. But there is not much evidence of its having turned this brilliant scientific capability into economic advantage. Japan, on the other hand, has received very few Nobel Prizes. Great Britain has probably received at least 10 times as many. Yet Japan's failure to receive Nobel Prizes has clearly not been a major economic handicap in the past.

Rather, the Japanese experience forcefully demonstrates the remarkable possibilities for economic growth based upon the systematic transfer and exploitation of foreign technologies. It seems obvious from the Japanese experience that, given the appropriate managerial, engineering, and organizational skills, a high degree of economic competitiveness can be attained by drawing upon more advanced technologies that are available abroad.

To put it the other way around, it is easy to exaggerate the purely economic significance of a first-rate domestic scientific research capability by itself. To be more precise, such a capability is certainly not a *sufficient* condition for economic growth. It is almost certainly becoming a more frequent *necessary* condition, partly because earlier access to new research findings can be translated into a lead time over one's competitors that may offer great commercial advantages. Nonethe-

less, I continue to be impressed with the number of instances in the recent past in which it has not even been a necessary condition for the Japanese.

This should not be terribly surprising. The fruits of purely scientific research have always been highly portable. What I am suggesting here is that events of recent years are rendering the findings of technological innovation highly portable *internationally* as well, at least under the right set of circumstances in the recipient countries.

The right set of circumstances consists of the cluster of capabilities associated with the rapid commercialization of new products or processes—in other words, the skills that lie further downstream from purely scientific research capabilities. I am thinking of the process of product design, of determining the appropriate trade-offs between product cost and performance, of the engineering skills involved in installing and organizing the flow of work utilizing new manufacturing processes, and so on.

At the macroeconomic level, the right set of circumstances means a stable economic environment that is conducive to investment activity, and especially to the willingness of people in industry to make decisions on the basis of *long time horizons*. Of course, there have been a number of forces in American society in recent years that have pushed us in the direction of industrial decision making based upon very short-term considerations. Let me expand on this point.

It has become fashionable in recent years to deplore industry's excessive preoccupation with short-term measures of economic performance. American management and business decision makers generally have been criticized for their narrow, bottom-line mentality—one that is focused no further down the road than the next quarterly report.

It is no part of my intention to offer an apologia for myopic business decision making. On the contrary, I would suggest that there are some compelling forces in the American macroeconomic environment that have been strongly con-

The American economy has operated with short time horizons, which is to say that it has heavily discounted the future in favor of the present.

There really is no such thing as meaningful science policy independent of economic policy.

ducive to this kind of myopia—to a preoccupation with this kind of short time horizon. Criticisms that fail to recognize such forces while simply deploring the short time horizons are, at best, superficial.

Moreover, I would like to suggest that the fundamental reason for short time horizons is also intimately linked to the reasons for the unacceptably low growth in American productivity in the last 15 or 20 years. The slowdown in American productivity growth can be very simply stated. According to the U.S. Department of Labor, the growth in real product per employee hour between 1948 and 1973 was almost 3 percent. It declined very sharply after 1973, and it averaged 0.6 percent for the period 1973 to 1981. It has recovered from the poor performance of the 1970s and grew at about 1.4 percent for the period between 1981 and 1985. But this is still less than half of the level that prevailed over the years 1948 to 1973.

The underlying macroeconomic facts that connect business myopia and slower productivity growth can also be simply stated. In a variety of ways and for a variety of reasons, the American economy in recent years has been generating low rates of net saving and capital formation. These rates are far lower than those of our main overseas competitors. The American economy has operated with short time horizons, which is to say that it has heavily discounted the future in favor of the present.

But this is the inevitable consequence of being a society that devotes only a very slender amount of resources to savings. Rates of net saving in America have declined very sharply in recent years. From annual averages of about 7.0 percent of GNP in the 1950s, and about 7.5 percent for the 1960s, net savings declined to barely 6 percent in the 1970s, and have declined much more sharply in the 1980s. Between 1982 and 1986, net savings averaged 2.3 percent of GNP, and in 1986 they were 1.9 percent. That is most emphatically no way to run an expanding industrial economy in a

highly competitive world. The Japanese net savings rate in the 1980s has also declined compared to the 1970s, but their rate has declined to just over 17 percent.

I cannot overstress the importance of those figures. A high rate of savings translates into a low cost of capital. A low cost of capital in turn translates into long business time horizons. Conversely, a low rate of savings translates into a high cost of capital and a corresponding business myopia.

In order to appreciate the full economic significance for American productivity of these lower levels of saving and investment, it is vital to point out a connection with another postwar trend of tremendous importance. I refer to the rapid rate of growth in the size of the American labor force. The ability of the American economy to generate new and enlarged employment opportunities in order to accommodate the postwar baby boom and the rising participation of women in the labor force is one of the most remarkable, if unsung, economic achievements of the postwar period.

In the 1970s alone, the employed labor force in America grew by no less than 20 million: from 80 million to 100 million between 1970 and 1980. For the 30-year period from 1955 to 1985, the American economy created 45 million new jobs. To provide some basis for comparison, the European Common Market countries created 5 million new jobs during the same period.

That is the good news. The bad news is essentially the other side of the coin: that we really could not afford all the good news! That is, the absorption of this flood of new entrants into the labor force—many of them with little or no previous job experience or training—meant a drastic slowdown in the growth rate of labor productivity. The slow growth in U.S. labor productivity is, in a fundamental way, a consequence of the rapid expansion in the employed labor force. However, it is only by directly juxtaposing these two separate trends—low rates of saving and investment and a high rate of new entries into the labor force—that the full magnitude of America's growing problems in international competitiveness and slow productivity improvement become apparent.

A major determinant of labor productivity is what economists call the capital/labor ratio. To a great extent, improving labor productivity is a matter of equipping each worker with an expanding stock of machinery and equipment. Of course, when the labor force itself is growing very rapidly, high rates of capital formation become even more important in providing a basis for continued productivity improvement. It should come as no surprise that there is a very high correlation internationally between a country's growth in labor productivity and growth in its capital/labor ratio.

In these respects, the comparative strength of the American economy has deteriorated rapidly over the past 20 years or so. The capital/labor ratio grew by 3 percent a year in America between 1948 and 1973. But after 1973, it slowed to an annual growth rate of less than 2 percent. In sharp contrast, the annual amount of fixed investment per manufacturing worker in Japan is more than twice as high as in America. In fact, that difference is growing rather than shrinking.

In the present context of rapid technological change and, more especially, the rapid international diffusion of technology, the implications of these purely quantitative trends become even more serious. This is because the ability to exploit the new technological possibilities generated by research and development depends in great measure upon the availability and cost of new capital. The speed with which new technological possibilities will be embodied in new forms of machinery and equipment, and the speed and extent to which firms will adopt these new technologies depend very heavily upon rates of saving and investment.

In closing, we should note that this analysis has obviously not presented the whole story. The American economy has been subjected to an exceptionally high degree of volatility in the 1970s

and 1980s that I have not mentioned. Outside events such as the Arab oil embargo and the run-up in energy prices were highly disruptive and led to great economic uncertainty. This translated into continuing fear of future inflation and led to erratic changes in government monetary and fiscal policies. This, in turn, led to an even greater reluctance to make long-term commitments in industry.

The resulting preoccupation with short-term problems has contributed to an economic environment inhospitable to the stability required for carrying out long-term R&D strategies.

One unavoidable conclusion is that the impact of science, and of R&D activities generally, cannot be meaningfully discussed outside of an economic context. Insofar as concern with scientific research is generated by its possible contribution to economic competitiveness and growth, we must recognize that those contributions are directly shaped and mediated by conventional economic variables. In effect, there really is no such thing as a meaningful science policy independent of economic policy.

DISCUSSION AND COMMENTARY

Q: As a scholar of nineteenth-century British economics you have written extensively on the degree to which nations in that century were able to make use of other countries' inventions to advance their own industrial well-being. Specifically, you have written that the United States gained a competitive advantage over Great Britain by making use of two British inventions—the turret lathe and the milling machine—to build a strong U.S. industrial enterprise at a time when the British were unable to make use of these products of their own R&D activity. Are there parallels here with the current situation in the United States?

NATHAN ROSENBERG: In spite of much discussion of Yankee ingenuity—of which there was a great deal—it is still true that America in the nineteenth century achieved its extraordinarily rapid growth largely through the transfer of technology. One might almost say that the United States was the Japan of the nineteenth century. We got our basic metallurgical technology, our iron and steel technology—the steam engine and machine tools—from the British. In fact, all of the fundamental technology that underlay the industrial revolution came to us from Great Britain. We improved upon it in many ways: we modified it and did a lot of inventing of our own. But, in essence, we were rapidly developing an industrial society based upon transferred technology.

With respect to the situation between Japan and America today, I would like to make two observations. First, Japanese R&D expenditures as a percentage of GNP now slightly exceed those of the United States. Japan's commitment to basic science, which has been relatively modest in the past, is growing very rapidly. Also of particular interest is the fact that a considerably larger share of total R&D expenditures in Japan is financed by private industry. That is to say, Japanese private industry has a vision of R&D as an important contributor to future profitability.

Of course, in comparing spending patterns in Japan and the United States, we should keep in mind the dominant role that defense-related R&D, which is overwhelmingly government financed, plays in the American case. In contrast, nearly all Japanese R&D is civilian in nature. Nonetheless, even if defense-related R&D is excluded and only civilian R&D is considered, the proportion of privately financed R&D in Japan is still considerably larger than in the United States.

Q: You have emphasized the present portability of research results, yet you did not mention patents at all. What is the importance of patents in U.S. technology today?

NATHAN ROSENBERG: That question has to be answered on an industry-by-industry basis. Obviously, in some cases patents are very important. For example, the pharmaceutical industry

would not function anything like it now does in the absence of a patent system. On the other hand, patents are a good deal less important in the microelectronics industry of Silicon Valley. This is true, in part, because the sheer rapidity of changes is so great that it outstrips the patenting process, and in part because what is most important in that industry is often simply a matter of design, the nature of which is not readily patentable.

But, clearly, one of the things that private industry is most concerned about is establishing and maintaining its proprietary interest in given technologies. Without that proprietary control, the commitment of financial resources to technological change would probably be much less than it is. Again, however, a detailed answer to this question would have to be industry-specific.

Q: My understanding is that while white-collar productivity in the United States has been quite low, blue-collar productivity has been relatively good, so that our factories are not in as bad a shape as the overall numbers indicate. Can you comment?

NATHAN ROSENBERG: The numbers I gave were of a highly aggregated nature. If you look at different sectors of the economy, you might get some very different answers. For instance, in considering Japanese productivity, the numbers are very impressive if you are talking about manufacturing productivity. However, if you look at productivity of the economy as a whole, the answer is distinctly different. For example, the Japanese distribution system is, on the whole, a remarkably inefficient enterprise compared with the American one. But within the manufacturing sector, I have seen numbers that tend to confirm your point.

For instance, output per blue-collar automobile worker in Japan and the United States is similar. In other words, the differences in blue-collar labor productivity in the Japanese and American auto industries are not all that great. On the other hand, the American automobile industry does seem to have a very large white-collar labor force. What it is that all of those people do I am not entirely sure, but there may be a great deal of improvement in productivity needed there.

Q: There is another factor contributing to the short-term focus of business management in the United States that you have not mentioned: the short-term focus of stockholders, particularly institutions with large shareholdings and thus a strong influence on management. Can you comment?

NATHAN ROSENBERG: In part, of course, the short-term orientation of stockholders generally reflects something real out there in the market and there certainly does seem to be a general preoccupation with the short term.

On the other hand, I feel that this phenomenon is closely connected with the low rates of saving and capital formation that the system as a whole is generating. Japanese investors seem to be much more oriented toward the long term in ways that reflect something about the capability of the Japanese economy to generate high rates of current saving. That simply has to be reflected in the overall price/earnings ratios and in current rates of return that corporations insist on in making future investments.

RICHARD MAHONEY: There are lots of share owners who clamor for instant gratification, but I have seen a number of successful R&D-driven companies that are, if not encouraged, at least tolerated. Sometimes we hear that our focus is too long-range, but just as often, when we announce something important, the same people say it is a splendid investment of their assets. I think the great bulk of R&D-driven companies are not harassed excessively by their share owners—providing the companies are reasonably successful.

Q: What kinds of government policies might encourage more long-term investment, a higher rate of savings, and a lower cost of capital?

NATHAN ROSENBERG: During the last 15 years or so the nation has experienced very sharp and unanticipated policy changes. Such drastic changes, especially those that are difficult to foresee, are very bad for businessmen trying to make commitments involving long time horizons. It seems to me that major changes in monetary and fiscal policy and revisions of the tax structure every few years, quite independent of the particulars of these changes, have had much to do with the high degree of uncertainty in our economy during this period. When people are uncertain, they tend to focus on activities with very short-term payoffs. Thus, encouraging long-term investment will require policies that, in general, provide for a higher degree of stability in the overall economy and avoid drastic changes in response to short-run crises.

For example, our tax structure in many ways has an antisaving bias built into it. In the long run we should be thinking about a tax structure that taxes consumption as opposed to taxing savings.

Let me give an example. In Japan, the interest derived from household savings is essentially tax-free. You can go to any post office in Japan and set up a savings account. When you reach a certain level of savings that you are not supposed to exceed, you can open another account in the name of a member of the family.

Even more significantly, you can go to another post office and open a new account, because it seems that the Japanese have made a very deliberate policy of not computerizing any of this. One post office does not appear to know anything about the accounts that are opened up in other post offices. That seems to me to be a very clever aspect of Japanese government policy. The result is, essentially, a much stronger incentive to save. The net payoff is a great deal higher than in the United States, where you pay taxes on interest generated by savings. So it is possible to introduce a variety of changes in our tax structure that would give greater encouragement to savings than the present system does.

Q (to David Packard): You have spoken about the importance of teamwork, not only within a company, but among different companies within a country. Is that the solution to the high cost of capital? Do we need more mergers like GE and RCA? Or do we need to change our economic policies?

DAVID PACKARD: This is not a simple problem; there are many factors involved. My reference to teamwork had to do with the importance of developing effective management. The availability of capital and these other issues are independent of that. It is very important to develop a management capability to couple research and development with manufacturing and marketing; it is important to have teamwork at all levels within the company. Whether capital is available or not, that kind of teamwork will result in better performance. But it really has no direct effect on the availability of capital.

However, I might add one comment. The question has been asked how the nation's low rate of savings and short-term vision have come about. My view is that, in part, they have been caused by a wrong focus of our business schools. These institutions have been teaching finance and leverage and wheeling and dealing, and I think they have done us a disservice in this respect.

Q: Do you think that we should relax antitrust laws a bit to allow more consolidation? Does that relate to the economic problems you were talking about?

NATHAN ROSENBERG: It is not directly related to the points that I was emphasizing. I do not think that we need a drastic rewriting of our antitrust laws, although we certainly might do some things that would, for example, make it possible for firms to do joint research of a kind without worrying about the antitrust implications. But the question of productivity and competitiveness is really a multidimensional problem. It is a matter of teamwork, of the savings and capital formation I mentioned, and even of the sorts of things we

teach or fail to teach at business schools and other places.

Q: Do you foresee any different R&D strategies—national or international—for the so-called commodity industries, such as energy, commodity chemicals, and plastics, as opposed to those for high-tech industries, such as biotechnology and microelectronics?

NATHAN ROSENBERG: There is a good deal of R&D going on today in many commodity industries—plastics and energy, for example. There are also industries in which a lot of technology arises through a kind of technology transfer, but not of the kind normally referred to. It is an applications-oriented transfer—developing new uses for existing technologies in industries in which they are not yet being applied.

Information processing technologies—computers—provide a good example. In the last 10 to 15 years, the American economy has undergone the computerization of many of its sectors. Computerized techniques have shown up in textiles, forest products, and many other areas in which you might be least inclined to look for this kind of technology. Anything that can be done to speed up this kind of transfer would be an important step. Whether this requires some kind of new government policy or not is not obvious to me.

REDUCTION TO PRACTICE: THE DEVELOPMENT AND MANUFACTURING CYCLE

RALPH E. GOMORY
Senior Vice-President and Chief Scientist, IBM Corporation

We live in a period of enormous excitement in science and in technology. Events of the past four decades have demonstrated that science, in addition to being a great intellectual pursuit, also has enormous practical power. The stunning appearance of the first atomic bomb clearly demonstrated that fact. The atomic bomb was the result of the gradual buildup of knowledge about atomic structure and the nucleus over the previous 40-odd years. This cumulative theoretical work engendered a very real physical mastery that in its first application put an end to World War II. In subsequent refinements it has given us the less desirable power to end the world.

A second example of science's great practical power was the appearance and rapid evolution of the transistor. Again, this was the result of the steady buildup of scientific knowledge, in the form of quantum mechanics and solid-state physics, during the decades since the 1920s. That development—in the manifestation of silicon chips—is rapidly transforming the world around us.

Even more recently, science's potential power has been demonstrated in the spectacular success of molecular biology. In its practical manifestation as biotechnology, it is well on the way to affecting living beings themselves in a profound and transforming fashion. Thus, in a relatively short time, science has demonstrated its power to destroy the world, to create machines that can execute 100 million operations in a single second, and to probe the very structure of life.

These stunning examples of the utility of science have led to a fruitful national consensus in the United States that has supported the scientific endeavor. Consequently, this country has been the dominant scientific power of the world since the Second World War. But to the surprise of many, dominance in the research laboratory has not turned out to mean dominance in products. Automobiles, steel, and, more recently, semiconductor memory devices have shown that dominant science does not necessarily translate to dominant industry, even in the most high-technology areas.

But we should realize that it never did. Both before and after the Second World War, the United States was the major industrial power of the world, and this was quite apart from the devastating effect of the war on the other industrial powers in Europe. For example, in the 1920s, the United States produced more than two times the iron, steel, and electricity of France, Germany, and Britain combined, and also more than twice as much per capita. It was on this massive and efficient industrial base that the overwhelming air and sea armadas of the Second World War were built. And all of this was done on a negligible domestic science base. The capitol of science in that period was Europe, but the capitol of industry was the United States. It can be said with some truth that we were the Japan of that period.

If dominant science does not necessarily mean dominant products, then what is the relationship between science and products? It is first necessary to realize that the leading role of science and scientists in the transistor and biotechnology examples provides a paradigm for the introduction of radically new technology. However, it is not necessarily a paradigm for the more ordinary processes of product improvement.

The process of introducing novel technology can be called the *ladder process*. It is the reduction to practice of a brand new idea. As in the case of the transistor or biotechnology, the new idea is dominant and the product forms itself around that new idea or new technology. Those who understand that idea or technology are often scientists, and they therefore play leading roles in its introduction.

However, there is another important process called the *cyclic development process* or the process of repeated incremental improvement. In this type of process improvement, an existing product—not a new one—evolves, getting better and developing new features year after year. Although this may sound dull, the cumulative effect of

these incremental changes can be profound.

It is this process of incremental improvement that, after the initial ladder-style introduction of the first transistor, has each year given us larger and better computer memories. In the past 20 years of incremental improvement, we have gone from one bit on a computer chip to one million bits. Incremental improvement is also the process that continues to provide us computer screens with better resolution and quieter, better quality printers.

This process of gradual improvement is, in fact, enormously important. A majority of the products sold today were here in a slightly inferior form last year. Most competition is between variants of the same product: my automobile against your automobile, not my automobile against your helicopter. In those areas in which the United States has failed to be competitive, it has been beaten not by radical new technology, but by better refinements, better manufacturing technology, or better quality in an existing product.

Let us examine this incremental product improvement cycle. Certainly, it varies a great deal from industry to industry and the description here will apply more to some than to others, but there is much that is common to all.

First, as mentioned previously, the world of incremental product development is, by definition, a world built around the existing product. It is not built around a new idea, as in the ladder process. Therefore, those in the best position to improve the product are those already involved in developing and manufacturing it, because they know the product best. In addition, what they can do to improve the product is strongly affected by what that product already is. This may seem obvious, but it has profound implications.

Second, there is the cyclic nature of the product improvement process. In the world of computers, printers, and computer displays, as well as in many other industries, a development team is working on the next generation of a product while the current version of the product is still being manufactured. Then, when the next generation is put into production, the development team begins work on yet another product generation.

These successive rounds in the process of improvement define the development and manufacturing cycle. It may be a one-, two-, or three-year cycle, or it may be longer still. For example, a company may currently be manufacturing 256,000-bit memory chips while engineers are working on the design and manufacturing process for the 1 million-bit chip—the next product round. When that 1 million-bit chip is ready, it is introduced gradually to manufacturing, which slowly builds production and phases out the 256,000-bit chip. Then the development process starts over again on the 4 million-bit chip. A similar cycle of production, improved product, and new production applies to printers and many other product areas.

One consequence of this cyclic process is that the speed of the development and manufacturing cycle is absolutely vital. If one company has a three-year cycle and another has a two-year cycle, the company with a two-year cycle will have its process and design into production and on the market one year before the other. The one with the shorter cycle will appear to have newer products with newer technologies. In fact, both companies will be working from the same storehouse of technology, and the same set of ideas. It is the speed of the development and manufacturing cycle that results in the appearance of innovation or technical leadership. It only takes a few turns of that cycle to build a commanding product lead.

A key factor in the speed of the cycle, as well as in its quality and cost, is the relationship between development and manufacturing. A product design that can be easily manufactured results in a rapid start-up of production. Close ties between manufacturing and development translate into early knowledge of technical problems, and thus into speed of product introduction and higher quality; a lack of such ties does the opposite.

An example will illustrate these points. One of

IBM's products is a wire matrix printer—the kind commonly associated with small personal computers. On inspecting these printers, you will find no highly sophisticated technology. They are all fairly simple mechanisms and wires. Thus, product quality and reliability tend to be of greatest concern to the development team. They must coax as high a print quality as possible out of the wires that form the characters and assure that they won't fatigue—even after 100 million impacts on ribbon and paper.

IBM was not very successful originally with this type of printer. In looking at competitive products, we found that they were much better designed for manufacturability. For one, they had fewer parts. So a combined development/manufacturing team took up the challenge of redesigning the IBM printer. The goal was to bring down the number of parts from about 130 to 60, and to eliminate screws. Why eliminate screws? Because screws are relatively difficult to use in assembly as opposed to snapping parts into place. Another goal was that all parts should snap from the same direction. This suggestion was made by the manufacturing people, based on their knowledge of what the machinery in the plant was best suited for.

In the end, the challenge was met. The final printer ended up with 62 parts and no screws, and it was very manufacturable. In fact, it became the best-selling printer in the United States at the time. This was directly attributable to that close tie between development and manufacturing and their joint focus on making something manufacturable.

Another important feature of the cyclic development process is its relative imperviousness to ideas coming from outside itself, that is, from people who are not participants in it. This bears on the earlier question of the relationship between dominance in science and success in the marketplace.

If you want to get new ideas into the development and manufacturing cycle from the outside,

In those areas where the United States has failed to be competitive, it has been beaten not by radical new technology, but by better refinements, better manufacturing technology, or better quality in an existing product.

timing is crucial. There is only a relatively brief moment when the cycle is receptive. You must propose these ideas at the *beginning* of the cycle, so you had better know when the beginning is. Halfway through is too late. If you propose a better head for a printer halfway through its two-year cycle—that is, one year after they have started the current round of product development—it is basically useless, no matter how good the proposal. The company is not going to interrupt the cycle, delaying the whole project by a year and thus ending up with a noncompetitive product.

Furthermore, the idea proposed will need to be largely fleshed out and tested so that the development team can expect to finish their work on time. For example, even if the new printer head is available at the start of the cycle, it must have been proven enough to give the development team the confidence that they will not disrupt the cycle time with its introduction.

Another impediment to new ideas from the outside is that a product is often so complicated or uses processes so complex that they are not completely understood. There are many examples of this: electroplating baths, whose composition and effects are not completely known, or the complex reactions of ions in a plasma over the surfaces of semiconductors. It is a fact in development and manufacturing that often you do not know exactly how something works, but you know that it did work last year. In such a situation, small evolutionary changes are more acceptable than large radical ones.

All of these considerations stem from the fact that the product already exists and is merely being refined. The nature of the product, its complexities, what the development and manufacturing teams themselves know or do not know—these are factors that frequently dominate the improvement cycle. Yet these factors are often only understood by the development teams themselves. It is not like the transistor, where a new thing was built around a new idea. This characteristic weighs heavily against ideas from the outside, especially those that are not yet highly developed, as is often the case with concepts coming out of university laboratories.

If new ideas are difficult to introduce into the development and manufacturing cycle from the outside, then those within the cycle who understand it best must be the bearers of these new ideas. This means that the product engineers themselves must be aware of all the relevant science and latest technologies. There is really no other option. If they are not up on what is happening technically in other companies or in the great research universities, a good deal of technology in the infrastructure will go to waste or, more likely, be seized upon by a competitor.

In this light, such activities as traveling to attend scientific meetings, reading the technical literature, being an active part of the engineering community, and keeping in touch with the universities are not frills. Neither are they indulgences given by management to the professional ambitions of the individual engineer. They are *necessities* if we are to compete with others who make those same efforts and are then able to rapidly incorporate change into their own complex product world.

Our most effective foreign competition to date has been characterized by close ties between development and manufacturing; an emphasis on quality; the rapid introduction of incremental improvements known to all into the development cycle of a preexisting product; and a tremendous effort by those actually involved in the product cycle to be educated on the relevant technologies, on the competition's products, on the latest developments in university research, and on what is going on in the world. These are things at which we in U.S. industry must also learn to excel.

In closing, let us look briefly at some factors outside the development and manufacturing cycle itself, even though I regard that cycle as very hard to affect from the outside.

First, an in-house research (as opposed to development) organization can be very valuable and

can reduce the barrier to new ideas *if* it is closely linked to product development and manufacturing. With those close ties it can understand the progress of the cycle and can present new options at the appropriate time. In addition, these ideas will be fleshed out enough to have them accepted. Personal bonds between research and development and manufacturing people can also enhance the acceptability of ideas.

Gaining an entry for new ideas becomes harder the further away you are from those developing the product. Thus, it is more difficult to accomplish from a university base or from a government laboratory. This is not to say that industry cannot benefit from these resources, but it must reach out to do so.

Cooperative research involving more than one company has potential if the nature of the product development cycle is constantly borne in mind. For example, it is probably a great advantage to have such work performed by temporary groups made up of people who later return to their home companies with new knowledge, and there reenter the product cycle.

Still, there is a limit to how much we, as practitioners, can be helped from the outside if we will not be wise enough to help ourselves. The United States has been very successful in the past at the science- and scientist-dominated ladder process of innovation, in which a wholly new idea moves from research into a wholly new product. But there is no escaping the fact that we must also learn to succeed in the rapid, cyclic, engineering-dominated process of incremental product improvement. Neither process is a substitute for the other. We need both.

Dominant science does not necessarily translate to dominant industry.

DISCUSSION AND COMMENTARY

Q: Maximizing the ladder process would seem to require an increasing role for basic research institutions—such as universities—in commercial enterprises. How can industry and universities change their current relationship to increase

the effectiveness of the ladder process?

RALPH GOMORY: At the risk of oversimplifying, I think the universities and the research community in the United States are doing a super job. They have been extremely effective at the ladder process, putting out one idea after another to generate new products. Unfortunately, once these products get rolling and become major industries, they become more the subject of the cyclic development process, and it is in that realm that our weakness has shown.

The way that universities relate to this is not easy to pin down. Frankly, I put the burden on the other component—on industry, which must learn to reach out to universities. Only industrial people know the details of their own processes. For instance, nobody knows the material we apply to the surface of the computer disk but us, and only we know what our problems are. We cannot afford to simply keep our heads down and concentrate only on getting the next product out. We have to have enough wisdom to look ahead and enough appreciation of the power of science to keep our people up to date, to send them to interact with the universities so that they can know the latest things and become the bearers of new ideas.

The Japanese make a remarkable effort in this area. Not only do Japanese engineers go out and listen to technical seminars at universities, they give talks as well—it is not a one-way street. They have very much the idea of being participants in the latest developments. There is also quite an effort on their part to send industry people to keep up with advanced work in America.

So I think we need a lot of industry pull, and I put the emphasis there more than on university push. The universities have created something of great value that we need to learn to exploit better.

NATHAN ROSENBERG: With respect to the interface between industry and the university community, there are changes going on every day before our very eyes. For example, we are seeing new organizational arrangements, such as the Center for Integrated Systems at Stanford, which is a specialized research unit supported by industry contributions and including the very active participation of industry representatives. In addition, the National Science Foundation has taken a leadership role in establishing engineering research centers at universities around the country. These centers focus upon very specific areas, such as new materials or information technology. So new interactions between the university community and industry are taking place.

But Dr. Gomory has told us, in essence, that these efforts will not really lead to very fruitful developments unless the right people from industry are involved in quickly integrating such activities into the product development cycle.

Q: How can we adjust our educational process to increase our engineers' and researchers' effectiveness at the cyclic development process?

RALPH GOMORY: I am not the first to say this, but I will add one more voice: we have deemphasized manufacturing in our curricula. We have failed to realize that manufacturing is an exciting area. It is not just a dull, grind-it-out business. Practically every kind of process appears in manufacturing. Often they appear on large scales, and often we do not understand them. For instance, we may not be able to accurately measure the flatness of something or how fine its particles are. We are unable to do a lot of things we would love to do in manufacturing, and that is where the challenge lies.

One way that universities can help us is by recognizing and promoting the role that science plays in manufacturing. We have to change our minds a bit and realize that manufacturing is as exciting and as scientifically challenging as the area of product development has generally been recognized to be.

Q: Isn't it possible that the Japanese have been successful, not simply because they have a better or quicker development cycle, but because they

have done better at generating creative ideas—have been more creative in the way they have designed automobiles or in the kinds of things they have done with electronic devices?

RALPH GOMORY: I am sure there are examples of losing out to better innovation on the part of our competitors. But, overall, we are pretty good at innovating; our weakness is more in other areas. Sadly, innovation is a little hard to measure. Is it the lack of a new idea that beat us or did someone else just get to market first? I have had the experience of seeing a competitive product come out and hearing our engineers say, "Oh yes, we know everything that's in it. We had all those ideas." Unfortunately, the competitor's rapid development cycle brought their product out first, so they are perceived as the innovators. But it is not due to a lack of ideas on our part; it is only a lack of their rapid implementation that gives the impression of a lack of ideas.

DAVID PACKARD: I would like to add a comment. We have not been completely remiss in this country in developing innovative ideas. The personal computer is a good example of this. Here was a case in which somebody completely outside of the computer industry made an important contribution—probably as important as anything that the Japanese have done. So we are quite able to have people come up with new ideas that break out of the conventional wisdom and add something important to our economy.

RALPH GOMORY: I could not agree more. We are very good at innovation; inventing the personal computer is the typical American thing. But let us see what will happen when it becomes a race not to have the idea—not to have the first personal computer—but a race to grind them out by the millions. That is more the question. This country has an enviable history of innovation. But the other aspect of industrial competitiveness—the grind-it-out, development process—is something that we need to improve.

THE ROLE OF MANAGEMENT IN R&D STRATEGY: A CASE STUDY

RICHARD J. MAHONEY
Chairman and Chief Executive Officer, Monsanto Corporation

How does a corporation's Chief Executive Officer view research and development as part of the corporate strategy? At Monsanto, research and development *is* the corporate strategy. I tell my colleagues that I have three strategic imperatives. The first is new products. They are the lifeblood of the company. After we have developed those products, my second imperative is *more* new products—this year, next year, and on into the future. Following that, my third priority is even more new products.

I remind people of those priorities whenever I get a chance because they reinforce a very basic fact. Whether we like it or not, Monsanto and other companies are not in business to do research, although we have some of the best and brightest scientists in the world. We are in business to take inventions—whether ours or brought in from somewhere else—and convert them into useful products that society wants and will pay for. That is the reality we have to keep in mind.

Few have the time today to do research just to scratch an intellectual itch. We expect research at Monsanto to be reasonably focused on areas of strategic interest to the company and to society. Like Monsanto, society expects a return on that investment in the form of an improved life. Industrial companies like ours can be an important vehicle for that return.

This tight focus on products obviously does not exclude basic research. At Monsanto, we do a substantial amount of basic research in areas we see as essential to our future. But we do not pretend that we are in business to make fundamental investigations into basic scientific questions. That is the role of universities and other institutions—a role that we support with dollars, moral support, and lobbying. Our primary role is to take science and technology and move them out of the laboratory and into the marketplace. That is what shareholders pay to see happen.

The importance of keeping R&D alive and moving can't be overemphasized. On a shelf behind my desk I keep a folder that lists every research project under way in the company. It keeps track of how much money we are spending on each project, when products are expected to emerge, when they will begin to pay money back to the company, and about how much we will have invested before the project becomes profitable. The folder is not a scorecard, and it is certainly imperfect, but it provides a flowchart that shows how R&D is proceeding and where we are relative to what we said we would do when we first allocated the funds.

Watching products move through the pipeline is exciting stuff. But it is critical to the success of any publicly held company that the R&D pipeline move promptly and profitably. So that folder also shows when a project is lagging or terminally ill. No research program has a right to immortality. Tracking progress is good discipline for making decisions about whether or not to proceed. Those decisions keep scarce dollars fueling the most promising programs.

I know better than to expect that I can dictate the pace of discovery or the certainty of success. Those powers do not come with the job. I also accept the fact that there are no guarantees in research. But it is my responsibility as Chief Executive Officer in a technology-driven company to select from a broad menu of research options. Therefore, it is also my responsibility to create an environment in which that menu can best be developed—an environment of direction broad enough and deep enough that imaginative scientists can create the future.

For example, in the agricultural biotechnology area, management might allocate $100 million over a five-year period to genetically modify soybeans—something that is very difficult relative to many other plants. But that $100 million is not a guarantee. It is a directional agreement. For instance, we might agree to pay $20 million to see the first card: development of the genetic transformation technique. But if you cannot meet the checkpoints and transform soybeans in the time agreed on, then the other $80 million is gone; we

will use it somewhere else.

Thus, it is a matter of paying for precise promises, especially when inventions are involved, and certainly when development timetables are required. It is like paying to see a card: when the card does not show up, ruthlessness is the only solution for a company that has more ideas than funding, which describes most companies.

On this kind of basis, we give our scientists some $600 million a year at Monsanto. That is about 8 percent of sales, and that is big league spending for our company. To put it in perspective, as recently as 1982 Monsanto spent only about 4 percent of sales on R&D. Last year marked the first time in the company's history that it spent more on research and development than it did on capital expenses.

Of that $600 million a year, we spend just over 25 percent on work to maintain existing businesses. We spend another 30 percent on expanding existing businesses. And we spend the remaining 40 percent or so on creating new business. That 40 percent is where the future will happen.

We don't underestimate the importance of work on existing products. In fact, we pay it great respect—and we have some of our top people working on it. That work is the foundation of our business, and we are doing good science. We've made breakthroughs in stain-resistant nylon carpet fiber, new grades and forms of engineering plastics, and new generations of sophisticated factory instrumentation. These products put cash in the till today and every day. That's a sound that every CEO loves to hear. Sales of traditional products will pay the bills and reward our shareholders for years to come.

But there is no doubt that basic research and early development stand high among Monsanto's priorities. Monsanto intends to prosper by creating novel products from scientific discoveries. That intention is based on the life sciences—crop agriculture, animal agriculture, and human health care—with a heavy emphasis on biotechnology. We are backing life sciences research with some two-thirds of our research dollars. We will continue to spend in that range in the years to come. That is a strategic decision that sets a course for the company for the foreseeable future. And it was not a decision easily made.

Monsanto has nearly completed one of the most thorough corporate restructurings of any company its size. We moved from being a company heavily dependent on commodity chemicals to a group of businesses that focus on high-value-added products. With such products our brains can count for more than the price of oil or the value of the dollar. Instead of being capital intensive, we are research intensive. Instead of being driven by external events, we are driven by products. Most of all, instead of being moderately successful, we intend to be very successful.

Our strategy began to emerge in the mid-1970s. At that time, Monsanto's sales were overwhelmingly chemical: fibers, polymers, industrial chemicals, and fabricated products. We were good at making them, and we made good money doing it. But the world was changing. After 30 years of growth, these familiar chemical markets were maturing. There were fewer new fibers, fewer new plastics, and fewer opportunities for these man-made products to replace other materials. Beyond that, oil companies and oil-producing nations were crowding into our markets with similar products made with competitive technology. In addition, the oil stocks at the time showed how fragile a position in commodities could be.

Monsanto had to change, so we began to look at our options for growth. New capacity wasn't the answer—there were more than enough petrochemical plants—and neither was patience. The good old days were gone, and they wouldn't return—at least at the profit levels we wanted. In the end, we concluded that even if Monsanto became the top performing chemical company, we wouldn't get the financial rewards for shareholders that we thought were appropriate. Chemicals—especially value-added chemicals with a large technical component—would be important,

but they alone wouldn't give us the long-term returns we wanted for our company.

When we fully realized that fact, we were free to look at other technologies. Instead of just trying to repeat the past, we began to look for ways to create the future. We were determined to find technologies that presented us with whole families of opportunities to exploit—research that led to ballrooms and not small closets. We studied instrumentation, diagnostics, sensors, and other areas. But we kept coming back to the life sciences. Three complementary trends at the time accounted for our interest.

First, we had a long-standing interest in moving into pharmaceuticals. It is a high-risk, high-reward business that could potentially meet our earnings expectations, and the technology is an extension of the chemical sciences we know so well. In addition, we know how to operate in a heavily regulated business environment. We saw pharmaceuticals as new and different, but not totally foreign.

Second, we had a strong and growing agricultural business. We were getting a good sense of that market and were developing a good sales organization.

Third, biotechnology was beginning to emerge as a promising new science. As a scientific tool, it opened up a huge range of opportunities—the ballroom, not the small closet. It is a technological link between our interest in pharmaceuticals and agriculture. From a common scientific root, we could develop in two areas of great interest to our company.

In pharmaceuticals, biotechnology created a new path to proprietary drugs. And because the technology was new, we were at the starting line with the other drug companies. We could compete on even terms, at least in that piece of the market.

In agriculture, biotechnology offered new ways to grow plants and raise animals—and to develop products for our worldwide marketing organization. It was a science of massive potential. In addition, our work on herbicides had given us a good foundation in plant science. We had a lot to learn—along with everyone else—but we weren't rookies.

Neither were we intimidated by molecular biology. We had been manipulating molecules for decades, and were used to thinking in those terms. The molecules in biology were, perhaps, more complex, but they were not outside the range of our understanding. In fact, we saw a lot of opportunities to combine biotechnology with traditional chemistry to produce the novel products we were looking for.

The more we looked, the more we saw an opportunity to make a technological leap forward—but a leap into familiar territory. By the mid-1970s, we were holding technical briefings on biotechnology. And in 1979, we alerted the Board of Directors to the potential need for a major new biotechnology laboratory complex. The strategy that guides Monsanto today was taking final shape, based substantially on the promise of biotechnology in the life sciences.

We cast a wide net for life sciences ideas. First, we invested in small, start-up biotechnology companies—like Genentech. That was the classic venture capital approach to opening a "window on technology." We gave money and management guidance and learned what we could.

Second, we formed innovative relationships with leading research universities: Washington University in St. Louis; Rockefeller University in New York; Exeter College, Oxford University in England. These relationships are still a strong and vital part of our program, with products beginning to flow from them.

Third, we built a powerful in-house research program. We constructed a $150 million laboratory and recruited a scientific staff as good as any.

With my strong view that the CEO should spend as much time with the Chief Scientist as he does with the Chief Financial Officer, I also got into the act. I spent one day a week for eight weeks in our biotechnology laboratories learning

Good research has to be supported by engineering skills, trained labor, financial investments, and marketing know-how. If we skimp on those investments, we'll have great technology and no products to sell. Someone else will sell them for us.

some of the basics of the science. I don't consider myself a molecular biologist, but those hours in the lab gave me a better feel for the decisions we had to make about our future in biotechnology. And I stay in touch with the bright young scientists I met to keep up with what they're doing.

As the pace of our activities in biotechnology increased and we gained momentum and confidence, we made other strategic moves to support our growing commitment to life sciences. We sold businesses with billions of dollars in sales and saved the money. We used that cash in 1985 to buy G. D. Searle and Company. We needed to buy a drug company to move some new products coming from our work with universities into clinical trials and then into the marketplace. We didn't have those skills in-house.

Today, Monsanto is at the end of a decade of change. Ten years ago, chemicals accounted for 75 to 80 percent of our sales. Today, they account for 50 percent, and those are mostly high-value, specialty products. The commodities are gone. In another 10 years, chemicals will total one-third of a higher sales base. The other two-thirds will come substantially from the life sciences research and technology under way today.

For the next decade, and on into the foreseeable future, R&D strategy will be the driving force for our corporate strategy. Our awareness of what was possible in the laboratory set us on this path. Our ability to deliver products from that work will determine our success. It has provided us with a menu of options. Now it's up to us to have the intelligence to pick the best opportunities for Monsanto and deliver results.

In the process of readjusting our corporate strategy we learned many lessons. First, research alone won't get us to the Promised Land. No company has more respect for good science than Monsanto. We absolutely depend on it. But we know there isn't a straight-line progression from big research budgets to successful products. Good research has to be supported by engineering skills, trained labor, financial investments, and market-

ing know-how. If we skimp on those investments, we'll have great technology and no products to sell. Someone else will sell them for us. That's a lesson this entire nation needs to learn.

And, we need support from government in the emerging technologies. I don't mean financial support. We'll risk the money. But we do need government to allow the research to continue and the products to be marketed. Regulations and legislation that needlessly delay important research cripple emerging technologies. The public demands protection—as it should. We need government to set safe regulations based on good science. Then we and the government must have the will to maintain those standards, and the courage to support the work.

Next, we learned that no single company has a hammerlock on technical excellence. Even with the best research scientists and the most efficient R&D programs possible, the flow of products from in-house work will be strong one year, weak another. In order to smooth out those ebbs and flows, a company has to be willing to look outside. We're not ashamed to license products and buy technology. One of the first people hired by our new head at G. D. Searle was a licensing expert. That's a familiar practice in the pharmaceutical business, and we're taking that flexible attitude to the rest of Monsanto. We need to keep in mind that our job is to develop products—not just to do research.

Finally, there are no ivory towers in industry research. Our scientists have to have a firm grasp not only of their technical specialties, but also of our business goals. They must understand our markets not only today, but also 5, 10, or 20 years in the future. Then they can apply their scientific creativity to marketing needs—or to new markets they can create. The reward isn't only in discovering new knowledge, but in transforming that knowledge into commercial success.

At Monsanto, we work hard to erase any difference between R&D strategy and corporate strategy. My favorite phrase to our commercial people is "take a scientist to lunch." To the scientists it is "get out of the lab and talk to customers; see it for yourself; draw your own conclusions." Market research into customer needs is a most imperfect science, but needs doing. And scientists themselves have a strong role. Today we are emphasizing biotechnology; tomorrow, other sciences will emerge that we will want to evaluate and, perhaps, be a part of.

Our purpose, then, at Monsanto is to meld good science and sound business strategy and to use that powerful synthesis to build the useful products of tomorrow—products that society wants. That's the business of industrial science.

DISCUSSION AND COMMENTARY

Q: An important factor in the long-term plan for Monsanto would seem to be strategic partnering of all types. You have mentioned Monsanto's affiliation with universities, as well as its role essentially as a venture capitalist in companies such as Genentech. How do you see the future of strategic partnering—especially between companies—as an element in your research and development?

RICHARD MAHONEY: I think strategic partnerships will increase. They will become not only more important, but *critically* important. When you think of the major unsolved issues in a field such as polymers, you realize no company has a lock on it; the collaborative opportunities are enormous. But there are some constraints. Typical joint ventures, in which each company shares equally in the effort, pose difficulties. Share owners often do not perceive much benefit from something that shows up as "other income net" in financial reports. That category is not usually where the stockholder focuses when he or she looks at the value of a company, yet that is where this type of 50-50 partnership unhappily shows up.

I believe ad hoc partnerships will probably be more the rule. For instance, this might involve a

marketing joint venture set up to be barely profitable. The two founding companies would bring their research into this venture and receive dividends back as venture owners, but the entity would not have a financial life of its own except to serve its masters and to reward the participants.

The massive collaboration that defines a classic joint venture is, I think, a very difficult proposition. There are some success stories—Dow-Corning and others—but not very many. And the problems are severe, the least of which is how to behave when the parent corporations change their minds, as they usually do, about their objectives. Such arrangements always start off like beautiful marriages, but the procedure for dissolution is usually the next step taken, so I am not fond of them. The ad hoc approach may be more effective, however.

Q: What is the internal process Monsanto uses to generate and select research projects?

RICHARD MAHONEY: The process is a very tricky one to try to describe—full of bootleg, and sometimes management outrage at first, followed closely by attention and loving care once an idea is seen to have some merit.

In assessing research ideas, low-cost trials or probes of the idea make good sense—keeping things small at first. We have made mistakes when we thought a project had potential and then tried to push it too fast. Even before the inventions had really been proven, the CEO was asking the question, "If I put three times as much money in it, can we make it go three times as fast?" The answer generally is "No, it will probably go only one-third as fast." The project takes on a life of its own, and so much is invested that the researchers are loath to admit failure. Projects like those just take too long to kill, so I like to keep things really quite small.

Secondly, Monsanto is trying some different approaches that may prove a useful model. We have watched venture capitalists for a long time, and have been exasperated at the time it takes us to accomplish some things as compared to venture capitalists. So we are trying yet another form of venture capitalism.

We have taken some people from our own laboratories and given them "founding shares" in a separate venture. We did not quite cut the umbilical cord—did not force them to meet the payroll or perish—but they are now participants in a separate company that we still own some 70 or 80 percent of. They are currently paper millionaires because they took some science that, at the time, Monsanto really did not want. Now they have gone public with a significant imputed value—probably more than the market has given Monsanto's entire R&D budget credit for.

This approach undoubtedly has its hazards, and I don't know whether it is going to be beneficial in the long run. But it does make use of the advantages of working with a small team. Unfortunately, in some of the biological sciences, research teams can be quite massive. Even so, I believe that the team approach is better than the system of one group doing a piece and then handing it off to a second group, which in turn hands it off to yet another group later on. If possible, I think they all should remain involved in the project until it is finished.

Q: With respect to Monsanto's cooperative research program with Washington University, how do you measure productivity for both basic and applied research projects?

RICHARD MAHONEY: Monsanto has a large multiyear program of research that it supports at Washington University. It does not distinguish between basic and applied research very much. A panel composed equally of Washington University and Monsanto people solicits proposals from scientists for funding; these research ideas can be basic or applied in nature. Sometimes it is hard to tell the difference in the fast-moving biological sciences. There is a certain percentage of the funds that is intended for fundamental research on biological structures. But frankly, most of the

people would like to get near a product because it is kind of exciting.

How do we measure productivity? In our case, we measure it by how many products are going to result from the work. From the standpoint of the university, I think they measure it by the quality of the research they are doing, the excitement of the environment that is created, and the kind of people they can attract, since they do other things than simply work on our projects. Those are the more important measures.

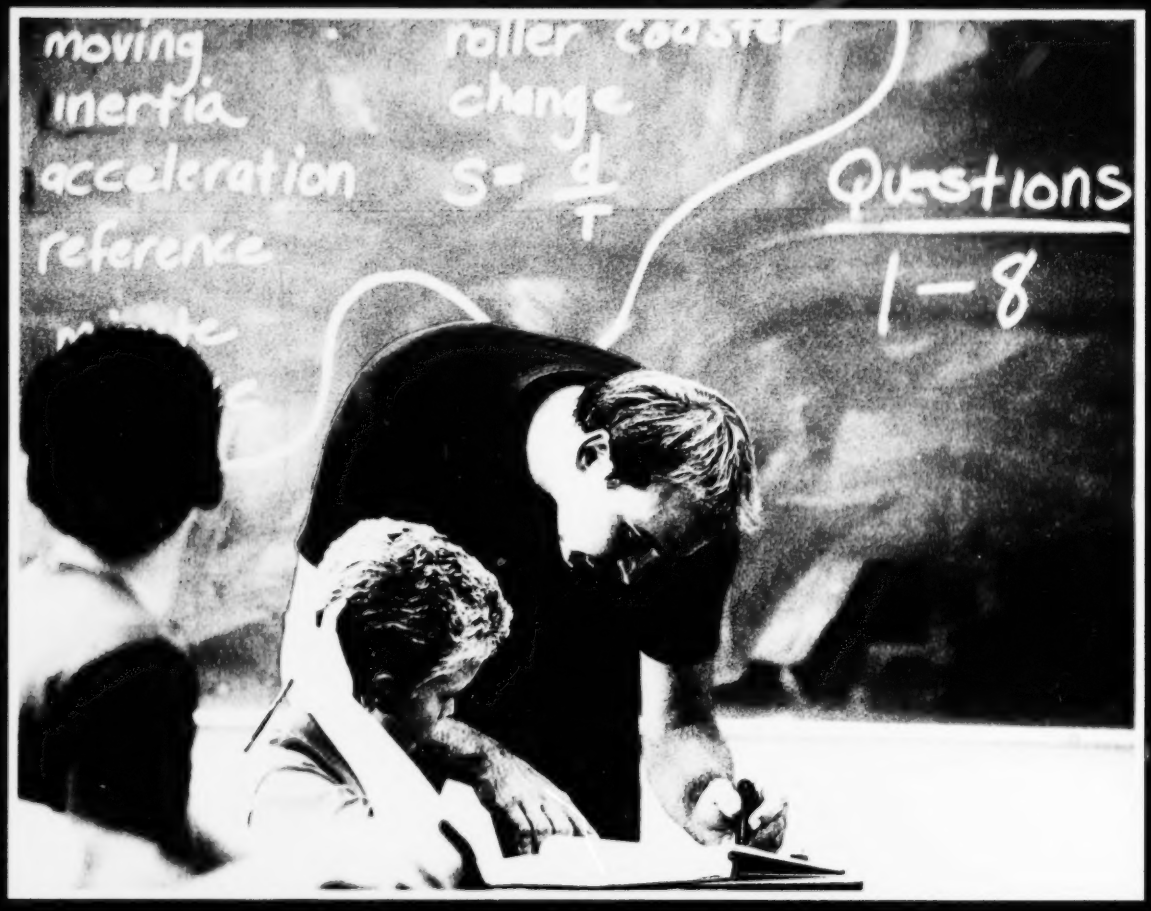
It is my experience with these investigators—even those involved in the most fundamental science—that they are on the phone daily to find out what stage in the development process the product is in. Even if they were only responsible for elucidating a fundamental mechanism, they remain vitally interested in the ultimate product. For example, a compound stemming from research at Washington University is now at a very exciting stage in development, and the scientist responsible for the early work is at least as interested in the product as I am.

Q: Several reports have suggested that in biotechnology the United States may face its stiffest challenge in the area of process technology for bulk manufacturing—once again, the manufacturing and development cycle Dr. Gomory spoke of. Can you comment?

RICHARD MAHONEY: The critical process step that will, in the long run, make or break many biotechnology products is not practiced very effectively anywhere. The basic process responsible for most of the proteins and peptides coming out of biotechnology is fermentation, but that technology is in long supply around the world. Rather, it is the separating out of the protein—the fishing out of the desired product from the fermentation mixture—that is the key technology. Nobody has that skill at the moment.

The Japanese, as you might expect, have recognized this. They are suiting up, funding projects probably entitled "The Fishing of Proteins Out of a Large Broth Cheaply and Effectively." Monsanto is doing what it can. We have started hiring anybody we can find that knows about the subject. We formed an ad hoc joint venture with a unit of Sandoz [a European chemical manufacturer]. We are building a plant in Austria now designed to produce a certain protein and, as I described, the critical step is fishing this protein out efficiently. In large-volume products, whoever does that best will win.

On the other hand, most pharmaceuticals are small-volume products. The cost of production is trivial as compared to the cost of research, and so in pharmaceuticals this may not be as big a factor. But, in general, I think one ignores this manufacturing process work at one's peril.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HUMAN RESOURCES FOR INDUSTRIAL RESEARCH AND DEVELOPMENT

ERICH BLOCH
Director, National Science Foundation

There is an old Chinese curse that says, "May you live in interesting times." These are indeed interesting times, but they are certainly not easy times. Yet we must live in them and somehow make sense of them. Amid today's economic turbulence—mirrored in the volatility of the stock markets and the declining value of the dollar—we must find the policies that will allow us to stabilize our economy and achieve steady growth.

Our task is to make the public and the political system view science and technology as *investment goods* rather than *consumer goods*. As long as they are seen as consumer goods—things that are nice if one can afford them, but not really essential—science and technology programs will expand in good times and contract in hard times. That is not an effective prescription for progress. Our need in science and engineering research, and also education, is to pursue a steady course, whatever it may be, rather than suffer the ups and downs that we have seen so often in the past.

It is wise to see science and technology as investments in our economic future. Steady support for them at a much higher rate than at present is thus an economic necessity. This is especially true of programs involving the education of people, for people are the ultimate resource. This is all the more true in a world in which ideas and technologies move easily from one place to another and technology diminishes the importance of "natural" resources.

The good news is that people are finally waking up to the problem. Recently, the Conference Board, the National Governors' Association, and the National Science Foundation conducted a joint survey among leaders of industry, universities, and state governments. Both business and university leaders ranked the availability and quality of technical personnel as the single most important factor in boosting U.S. competitiveness. The second factor was R&D resources.

These two factors were thought to be more important than trade policy, fiscal and regulatory policies, and even technology transfer. This consensus is encouraging because it will focus government, the states, academia, and the private sector in supporting policies that encourage education and research—two essential ingredients of economic competitiveness.

In the past several years, the need for competitiveness has become conventional wisdom. Nonetheless, stressing it is still worthwhile. Otherwise, there is a danger that competitiveness will go from today's good idea to yesterday's slogan so quickly that it may not have time to have much effect.

In part, our competitiveness problem is due to the shift from a national economy to an international one. This is a point that some of our European and other foreign colleagues grasp intuitively. But we sometimes miss it, because until recently—the last few decades at most—our economy was largely isolated from the rest of the world. Today, however, the world economy is thoroughly integrated. A glance at the stories on the business page about world stock markets will confirm this.

There are many causes of global economic integration, but perhaps the most important is a shift to an information- and knowledge-based economy. Access to natural resources and nontechnically skilled labor are no longer as important as they once were. In this new global market, knowledge-based industries built on semiconductors, biotechnology, synthetic materials, and the like are replacing resource-based industries—steelmaking, for instance—as the basic elements of the world economy.

Many of our major competitors understand this. As a consequence, they have sharply increased their investments in R&D and in science and engineering education. Some examples will demonstrate this point.

As a percentage of GNP, Japan now supports as much basic research in science and engineering as we do—about 12 percent. We may quibble about the definition of basic research, but that simply obscures the fact that Japan, in particular, is very

It is wise to see science and technology as investments in our economic future. Steady support for them at a much higher rate than at present is thus an economic necessity.

determined to make a better showing for itself in basic research.

Europe has also developed a wide array of programs in information science, telecommunications, robotics, and materials. We are, perhaps, underestimating the reawakening of Europe in scientific terms. A tremendous ferment is taking place there, with much investment and new cooperation between various governments, and also between industry and university or government laboratories.

We know now that rapid dissemination of research results speeds the product development process. In so doing, it also demonstrates that no nation can remain competitive without investing in new knowledge and translating it quickly into new products and technologies.

In order to accomplish this, there must be an infrastructure—people, equipment, and institutions—in place and maintained at the state of the art. Thus, we return to the issue of the human resources necessary for science and engineering. By far the most important element of a wise science and technology policy is people. People create the knowledge that makes technological innovation possible. They run our industries. They are the mentors and teachers of new generations of scientists. They are leaders and decision makers in our modern technological society.

Awareness of the importance of the human resource to economic competitiveness is reflected in other nations' investment in human capital. Japan has doubled its technical work force over the last 20 years. It produces about as many engineers annually as we do, but with half of our population.

In the United States, scientific and technical employment has grown three times faster than real GNP and twice as fast as overall professional employment. Professional employment, in turn, has grown much faster than total employment. Clearly, this suggests not only that scientists and engineers are in demand, but that this demand will rise as our modern knowledge-based econ-

omy develops further.

Will we be able to meet that demand? The answer is a resounding "No!" unless we change our policies. We are not generating the human talent we need. Our college-age population is shrinking. And in the last few years, a smaller percentage of college students is choosing to major in science or engineering.

The number of 22-year-olds—potential college graduates—has been dropping steadily and will continue to do so. Thus, unless a greater proportion of the undergraduate population is attracted to the science and engineering fields, the total number of science and engineering degrees will decline in the future.

With fewer and fewer young people, the importance of attracting women and minorities to science and engineering fields increases. They represent a large potential source of future technical professionals. Historically, however, these groups have had low rates of participation in technical disciplines. Despite significant increases in the number of women in graduate programs, they earn fewer than 15 percent of all technical degrees today. Worse still, recent increases in the number of women in science and engineering degree programs have leveled off at too low a point.

Minorities are especially important, since they make up an increasing fraction of our college-age population. They represent about 20 percent of the population, but account for less than 2 percent of doctoral degrees in the physical sciences and engineering. In the year 2020, they will account for a third of the total U.S. population. If we want more scientists and engineers, we must go where the resources are. This is not just a matter of equity; it is a matter of national need.

Over the last 10 years, there has been a significant decline in the rate at which undergraduate science and engineering majors go on to their doctorate. This trend has been leveling off in the last few years, but this stabilization has occurred only with the influx of foreign students into our graduate schools.

In recent years, more than half of our new engineering PhDs have been foreign nationals, and the proportion of foreign citizens earning mathematics and physics PhDs has increased significantly. Overall, foreign students account for nearly 85 percent of the recent growth of graduate education in the United States.

With fewer Americans choosing science or engineering careers, we are increasingly dependent on foreign nationals in some of the most important technical specialties. We are fortunate to have them. Many foreign graduates of our universities stay here and contribute to our economy and to research and education. But it is bad policy to depend on a resource that is not under our control. As opportunities overseas increase, we may find that the number of foreign students coming to our universities declines, and more of those who do come may return to their own countries.

Turning these trends around will be a large undertaking. The demographic trends are fixed, and attraction rates tend to be stable over a long period of time. The special challenge here to the academic community is to link precollege and higher education more closely. Our high school programs are often mediocre. Their curriculum and quality of instruction need drastic improvement.

It is not enough for those in the higher education community to simply observe this. They must lend their assistance. Our sources of excellence, namely, our best technological universities, must reach out to the high schools and small colleges at which excellence needs to be created.

If we fail to improve the situation, we could face a progressive deterioration of our technical skill base, a problem similar to the "brain drain" other nations have faced. If our leading industries fall behind, our best people will no longer be attracted to the critical technical areas. The economic payoff will no longer be there and we will fail to develop new skills or even to maintain those we have.

The semiconductor industry is a good case in

point. As we lose our leadership position to Japan, the attraction of the field will decline. Both students and venture capital will move to other areas. In the process, the nation will lose the leadership of a critical industry. We have seen this happen before. Nuclear engineering, for somewhat different reasons, suffered a similar fate. How many bright students today are likely to train for careers in reactor design or operation? Are we willing to depend on foreign sources for these skills?

Manufacturing is an especially important area in which we are slipping behind. We have not kept up with the technologies and strategies most needed today. Japan, obviously, has taken the lead by adopting a systems approach to manufacturing that involves the whole enterprise. They realized quickly that model manufacturing requires attention to much more than hardware and tools. Such factors as quality, inventory control, and the education and participation in decision making of the whole work force, including engineers, workers, and managers, are at least as important.

There is entirely too much talk about the decreasing importance of the manufacturing sector. That view is not supported by facts. Between 1965 and 1985, the dollar value of manufacturing worldwide increased 125 percent. It increased 80 percent in Europe and 600 percent in Japan, but only 50 percent in the United States. In 1965, the manufacturing level of the European Economic Community (EEC) countries totaled only about a third of the U.S. level. Today, the EEC countries exceed the United States in manufacturing capacity.

We are entirely too comfortable with the idea of writing off this sector. Both industry and academia can share the blame for this. Industry allowed manufacturing to become a backwater. The best people went elsewhere—to product engineering or to research. In the universities, bright young engineers realized that the way to tenure did not lead through manufacturing. The subject simply dropped out of sight—and out of the curriculum—at all but a very few schools.

Our society has two needs in technical training and education. One is the need of our industries, universities, and public sector for highly educated scientists and engineers. The second need is also vital, but less appreciated. It is the need for a scientifically literate public.

There was a time when the absence of a high school diploma and basic academic skills was not an obstacle to employment. That time is disappearing. Employment in the modern world increasingly demands basic literacy in science and mathematics. Thus, we should be concerned that in this country one out of four individuals does not finish high school. Nearly 30 percent of the nation's high schools offer no physics, 17 percent offer no chemistry, and 70 percent offer neither earth nor space sciences.

In comparative international tests, our students are not only outscored by those of other nations, they often place last; U.S. students rank in the lowest quartile in geometry, along with countries such as Chile, Nigeria, and Thailand. Our students rank well below those of some of our most important competitors, especially Japan and the Soviet Union. In the senior year, our high schools typically teach algebra, while Japanese and many European schools teach calculus.

As a recent National Science Foundation survey of public attitudes toward science and technology indicates, there is no lack of interest on the part of the general public. It is a lack of opportunity. The schools just don't have the quality programs our students need.

While our research scientists and engineers are the best in the world, too much of our population is unprepared to be productive in a complex technological world. The two needs cited above—for highly educated scientists and engineers, and for a general public with basic science and mathematics skills—converge on one point: the precollege level. A Nobel Prize for the cure for AIDS, new products for industry, and new weapons for the military all begin with the schooling of children.

Needs at the primary and secondary levels parallel many of the needs of higher education: better teachers, better pay, and better curricula. But they also include more demanding course requirements, higher standards, intensive teacher involvement, as well as the involvement of parents.

Meeting these needs will require a wide array of supports. These include teacher training modules, curricula development, better instructional equipment, access to well-equipped laboratories, and better communication between science and mathematics teachers and university faculty. This will be an enormous task, for which current levels of funding are inadequate.

State governments have been making important improvements in teaching standards and curriculums, and there is evidence that teacher salaries are beginning to go up in some places. Yet, important as this wave of reform has been, we need a "second wave" of reform, as Governor William Clinton of Arkansas terms it, that recognizes the central place of science and mathematics education in a high-technology economy.

The responsibility for continued reform in the schools lies mainly with the states, but it is not theirs alone. This issue affects us all, and each of us has a role to play.

The National Science Foundation, within its limited resources in this area, will provide more support for teacher training and curriculum development. It will also continue to support informal science education through museums and television shows.

Universities and industry, too, have important resources to bring to the problem. They should make their scientists and engineers available to schools to improve teacher preparation and course content.

In conclusion, since people are the basic resource, investments in people—in science and engineering talent—will pay enormous dividends to our society in the long run. But they must be made at every level, from primary through graduate schools, because educating scientists and engi-

By far the most important element of a wise science and technology policy is people. People create the knowledge that makes technological innovation possible.

neers is a continuous process.

Our educational system needs a major overhaul at all levels: elementary, high school, and even in some of the undergraduate areas. We must attract more students, especially women and minorities, to science and engineering. We must demonstrate to young people—especially to the brightest of them—that the rewards for careers in science and engineering are really there. This requires thriving industries that are actively challenging technological frontiers.

Can we do all these things? No doubt we can. We did as much or more for World War II and for the moon shot. But those were intense efforts directed toward relatively narrow goals, with payoffs expected in a matter of a few years.

What makes today's problem more difficult is that the solution requires many different activities, sustained over a long period of time. No one thing we do will have a dramatic short-term effect. One of the most difficult things for this country to do is to mount the sort of patient effort that pays off over decades rather than years. That sort of effort will not be easy, and it certainly will not be quick, but it is what is needed.

DISCUSSION AND COMMENTARY

COMMENT by STUART EIZENSTAT (Symposium panelist; legal counsel to the Council on Research and Technology and former Chief Domestic Policy Advisor under President Carter): The Council on Research and Technology was formed a year ago and is made up of about 40 corporations, 75 universities, and 16 high-tech companies. We have tried to identify an R&D agenda so that the nation can address some of the problems that Mr. Bloch has mentioned.

For instance, we are trying to make permanent the R&D tax credit. Last year, the U.S. Congress, in the Tax Reform Act, extended the R&D credit for 3 years, but reduced it from a 25 percent incremental credit to a 20 percent incremental credit. This took place against the background of the trends that Mr. Bloch mentioned, and at the very time when other countries have been increasing their incentives for R&D. We are now trying to hold on by our fingernails to keep the 20 percent credit and then make it permanent.

We are probably the only industrialized nation in the world that would have this kind of debate over simply maintaining, let alone increasing, an incentive for R&D through the tax code. It indicates the difficulty of forming a political consensus in favor of promoting R&D. Everyone is in favor of research and development, but when the federal pie gets allocated—and I have been part of drawing up budgets for a good long time—R&D does not have a constituency.

There is a well-organized constituency for just about everything else in the federal budget. Anyone dividing up that federal pie, either in the Congress or in the administration, hears from those groups. But when it comes to research and development and the sorts of long-term investments in education as were mentioned by Mr. Bloch, there is no coordinated constituency. I can tell you that at the White House level it is still true that the squeaky wheel gets the oil, and you just don't hear any squeaking from the R&D community.

Another area that the Council on Research and Technology has been involved with is supporting the President's current request to increase NSF's budget 17 percent this year and to double it over the next 5 years. In this way, NSF will be able to support some of the educational programs so badly needed and to deal with the R&D facilities problem on our university campuses.

When we talk about doing high-quality basic research, we must realize that more and more corporations, even those capable of performing top-flight research on their own, are looking to universities to take up a greater share of this cooperative basic research. Yet, according to several recent studies, university facilities are in shabby, second-rate condition. Universities are being called upon to do world-class research at less than

world-class facilities.

Nonetheless, we have had a great struggle with Congress on the NSF budget and the R&D Facilities Bill, which would establish a matching grants program for universities to build new facilities. This epitomizes the budgetary dilemma we face. We must make the kinds of investments in education, teacher training, and research and development that Mr. Bloch laid out, yet we are confronted with huge budget deficits pushing us in exactly the opposite direction.

Who gets cut? What gets reduced? Who has to bear the burden? Having participated with Congress in such deficit-reducing sessions as we have seen recently, I know that it is very difficult for members of Congress to make fine distinctions between programs that are investments for the future and those that are aimed at current consumption—in other words, to save R&D and to cut elsewhere.

What usually happens is that you cut across the board. Then you can say that everybody has had to bear their share of the burden equally. Unfortunately, you lose the ability to discriminate between what really will help the country in the short and long term and what we might be able to do without.

One of the nation's basic problems is that we cannot compete on the basis of our unit labor costs, which are among the highest in the world. And we cannot expect our workers to settle for the wages paid in Hong Kong or South Korea. We can only be competitive if we are more productive and more innovative. That will only occur if we are prepared to reorient federal resources.

We must convince our elected officials that investments in education and R&D are absolutely necessary to a competitive country. They must be made to realize that, as we go through this difficult period of reducing our "megadeficits," we should not only preserve, but increase investments in education at all levels—from Headstart through the postsecondary level. They must come to see that we have to not simply maintain, but increase R&D expenditures, both directly and indirectly through the R&D tax credit and similar vehicles. Only in this way will we find our way out of the current budget crunch.

We live in the most difficult budgetary situation I have seen since coming to Washington. It is not going to be resolved through any easy means. But if we choose to cut research and development in the name of reducing our budget deficit—whatever short-term benefit we may derive—it will lead to long-term disaster, and we simply cannot permit that to occur.

Q: As you stated, the United States once was preeminent in the field of nuclear engineering and the construction of nuclear power reactors. Lately, this field has fallen out of favor. Yet it may again one day figure in the nation's energy strategy, and at that point we may no longer have the necessary technical expertise available in this country. Do we face a similar risk in other critical technology areas?

ERICH BLOCH: At NSF, we are very much concerned with patterns in undergraduate and graduate education that may have significance for the nation's future technical capability. The history of nuclear engineering is of interest in this vein. At one time, nuclear engineering was pursued at a very high level in U.S. colleges and universities, but no longer. Students gravitate to subject areas in which there is a general interest, and that interest has waned in the nuclear field. Nothing we can do will change that situation if students don't see a future in the field—a career path that they can pursue.

Yet this concerns me. I fear for the nation's future in terms of attracting qualified people to those critical technologies and disciplines in which we will require a skill base. Because, at the same time, the industrial skill base that currently exists is, in a sense, "managing" us—determining what directions are possible for us in the future on the basis of what is in vogue today.

I think that pattern ultimately leads to dein-

dustrialization, which leads in turn to lack of capability at some future time when you really need it. So, I am concerned on a much broader scale than just what has happened to nuclear engineering; it just happens to be an interesting case study.

Let me also make a more general point. In nearly all of our nation's science and engineering affairs, we have neglected in the past to gain broad public acceptance. We have not educated the public as to our intentions: why the need exists, what the problems are, what the solutions are, and so forth. As a consequence, we don't receive public support at the point at which we really need it, and Mr. Eizenstat's comments reflect this.

We are not getting our story across, and that is the first thing that needs to happen, perhaps especially so with nuclear energy. In the past, we have not taken the time and the effort to educate people, or even to educate ourselves when it comes right down to it. But unless we have the public with us, there is very little that we can achieve by ourselves.

Q: NSF sponsors several programs to help minorities in education; some of these programs are many years old. Yet, as you have mentioned, in the past 10 years, the participation of college students in science is down, and the percentage of minorities going on to the PhD and beyond has shrunk. Should the success of NSF's programs be measured by an improvement in those numbers? Should NSF be doing more to increase minority participation in science and engineering?

ERICH BLOCH: The answer to both questions is yes. In fact, if you look back over the last 10 years, one of the problems has been that we did not continue these programs for long enough periods of time. We started them, then discontinued them and started something else. Equally wrong was the fact that, at the time we started the programs, we didn't establish proper evaluation criteria. We are trying to rectify both of these problems.

On the other hand, we can't say what would have happened if we had not had these programs. Maybe they were effective. Even so, the numbers that we are dealing with today are still very low and unacceptable. Obviously, we cannot rewrite history. But from here on, we can do two things to rectify the situation. First, we must be clear on what we want to get out of these programs and how we will measure them as they continue. Second, we must stay the course on them and not discontinue them after two or three years. In this area of education, attracting people to certain areas like science and engineering is a long-term proposition. You don't change it overnight.

COMMENT by SHIRLEY HUFSTEDLER (former Secretary of Education under President Carter): I have three comments on potential educational strategy to deal with the deterioration of the nation's science and technology infrastructure.

First, if we are to change the national mind on the value of education and where we ought to put our money for R&D, we have to remind citizens that national security and national defense cannot be limited to concepts of purely military defense against recognized military powers abroad. We must remind ourselves that Pogo's observation was right: "We have seen the enemy, and it is us."

This country will fall apart not from what is done to it from the outside, but from what we do to ourselves in emulating the worst of our enemies. In short, we ought to devote our energy to demonstrating that the way you build national security is to build the competencies, values, and team spirit that are already here in America.

Second, if you want creative people to work, the incentive method—the carrot—is infinitely better than the stick. Therefore, if you want to encourage creativity in education in a period when we know we cannot do everything, you reward the very best in the system—the very top of the structure.

You find gifted teachers in preschool, elementary school, and secondary education, and you find students who are doing well. When those

people receive tangible recognition and tangible rewards, they begin to lift up the whole structure with them.

Third, you devote your priorities to the youngest children in the society, because, of course, it is with those youngsters that you get the biggest reward for the most modest investment. Every additional increment of age means doubling and redoubling the resources that must be invested to tap the treasures within those children.

STUART EIZENSTAT: About the incentive system Ms. Hufstedler spoke of: the federal government has, at various times, made use of incentives to encourage a whole variety of activities. For example, we had incentives from the 1950s to the 1970s to encourage more physicians. What happened? They succeeded through specific dollars-and-cents carrots of the kind just mentioned.

It is not beyond our capability to have the same type of incentives for PhD students in engineering and science. In other words, if we determine that there is a national need for more scientists and engineers, we can institute the same sorts of incentives we established for doctors.

We are now going through a national debate about proposals like merit pay for teachers and the designation of so-called master teachers, and so forth. That is really the same concept of incentives. If we reward those people in particular professions that we identify as being critical to the national interest—for instance, if we give special bonuses and attention to math and science teachers—ultimately more people will gravitate toward those fields. This is a free market system, and it works by incentives.

I think our job is to translate that philosophy into policies at the state and federal level to provide these sorts of alternatives and incentives. I dare say that with a relatively modest set of incentives, the signal will be very clear. People in this country don't respond well to rhetoric, but they do respond quite well to financial incentives.

COOPERATIVE R&D VENTURES: COLLABORATION IN THE SEMICONDUCTOR INDUSTRY

WILLIAM F. BRINKMAN
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The challenge of surviving in an increasingly competitive world economy has spawned a number of novel cooperative R&D ventures lately involving industry, universities, and national laboratories. This is perhaps most visible in the semiconductor industry, in which competition with Japan is fierce.

Unlike the budding biotechnology industry, the semiconductor business is no longer dominated by the introduction of radically new products. Rather, the semiconductor industry is now in a stage typified by continuous development cycles of essentially the same type of product, with each new round having increasing complexity and integration.

It is an industry in which automated design, sophisticated processing equipment, and efficiency in manufacture have become the factors that lead to a competitive edge. This is exactly the type of market in which the Japanese frequently excel. Indeed, in the past several years, the periodic softening of the market, along with very strong competition from Japan, has put a number of U.S. semiconductor firms in serious trouble.

In three years, U.S. firms have gone from holding 64 percent of the overall semiconductor integrated circuit market to about 53 percent. In the specific area of dynamic random access memories (DRAMs), the U.S. share went from 100 percent of the market to less than 10 percent in 10 years.

These events have provoked much discussion about the possible demise of the U.S. semiconductor industry. There have been many reports on the subject, the most prominent of which was written by the Defense Science Board, an advisory body to the Defense Department. It examined the state of the U.S. industry from the viewpoint of national security and concluded that a healthy semiconductor industry was essential to the national well-being. It surveyed the different technologies that make up the industry, and pointed out that in a majority of these technologies the United States appears to be losing its lead.

The situation with DRAMs has already been mentioned, but static random access memory devices (SRAMs) are also at issue. The United States still leads in microprocessors and logic circuits, but we have fallen behind in memory and nonsilicon circuits. We are behind in optoelectronics, in which optical devices and electronic devices are combined to make more complex devices. We are also behind in heterostructure materials, that is, materials grown one atomic layer at a time in an ultra-high vacuum. Interestingly, we are even behind in the preparation of high-quality crystalline gallium arsenide. We lead in the ion implantation area, but we are rapidly losing our lead in electron-beam and x-ray lithography.

These facts have led to considerable activity in both government and industry to formulate programs that will not only allow the U.S. semiconductor industry to survive, but to regain its leadership. The fact that the Japanese government has acted aggressively to advance its domestic semiconductor industry has strongly influenced the proposals that have emerged. The Japanese have formed a series of industrial consortia to address various short- and long-range technology issues. Such consortia are generally funded for a specific length of time with monies from both the government and participating industries. The first such group began around 1980 with an initiative directed at design and production of very large-scale integrated (VLSI) circuits—the so-called VLSI initiative. Other examples besides the VLSI effort include consortia on optoelectronics, superconductors, and x-ray lithography.

Whether these Japanese consortia are particularly successful is subject to debate. Whatever else is true, Japanese companies have penetrated the integrated circuit market with increasing success since they formed the VLSI consortium. Certainly, some technologies were transferred from that project back to the industries involved. But when asked, many representatives of R&D laboratories in the Japanese semiconductor industry state that they would have been happier to see the money for the VLSI consortium spent in their

The semiconductor industry is an industry where automated design, sophisticated processing equipment, and efficiency in manufacture have become the factors that lead to a competitive edge.

own laboratories.

However, that view is perhaps to be expected from an R&D manager, and should not serve as the acid test of the consortium's success. More importantly, the consortium's activities have served to raise the Japanese national consciousness with regard to the technological challenges of the industry. This fact alone may be the essential role that such efforts play.

Now let us look briefly at the activities that are going on in the semiconductor industry in the United States. First, it is clear that there has been strong interaction among this industry, our universities, and the country's general research community for a long time. Clearly, the invention of the transistor, the conception of the integrated circuit, and the development of associated materials processing all came out of research labs throughout the country.

Universities such as Stanford, Cornell, the University of Illinois, MIT, the University of California at Berkeley, and the University of North Carolina have contributed significantly to the semiconductor industry. Ordinarily, these contributions have come from individual university professors whose research groups concentrate on work relevant to the industry's needs.

In recent years there has been a trend toward establishing research centers, such as the Engineering Research Centers created by the National Science Foundation. It is interesting to look at one such center founded by NSF a number of years ago: the National Nanofabrication Facility at Cornell. This facility was created with a relatively meager amount of money—on the order of $1 million a year from the NSF, and a sizable amount of money from other institutions and funding agencies.

I think the Cornell lab has been successful primarily because it was directed at a very specific technology and was provided with the kind of equipment necessary to lead that technology. In the nanofabrication area—the technology of fabricating structures smaller than a micron (one-mil-

lionth of a meter]—researchers using the Cornell facility have developed fine-line lithography techniques and dry-processing techniques very useful in creating very tiny structures. A number of companies can attest to this.

For instance, General Electric derived considerable benefit from their work on so-called hemp devices—gallium arsenide field-effect transistors used to generate microwave radiation. General Electric found that, rather than having to go out and buy all the expensive equipment to do the fine-line lithography necessary to make these devices, they could work with Cornell to develop the devices of interest and see if they were useful before they invested in the technology themselves. That was a very useful role.

At Bell Laboratories we also went to Cornell with essentially the same types of device ideas as GE and used the Cornell equipment to reduce the size and consequently increase the speed of the devices. In another instance, McDonnell-Douglas built their laser diodes at Cornell. These and many other companies will testify to the success of their collaborations with Cornell. Although the facility is primarily run on the basis of funding from government agencies, industry is attracted through corporate associate membership, with regular visits by industrial laboratory people.

The universities are not the only people who have been involved in this sort of activity. The national laboratories have also participated in many industrial collaborations. In fact, Brookhaven National Laboratory's neutron reactor has been used to explore much solid-state physics by observing the scattering of neutrons from solids. The interactions among AT&T-Bell Laboratories, Brookhaven, IBM, and others have been crucial to gaining these results.

In addition, Brookhaven now operates the National Synchrotron Light Source, which provides synchrotron radiation for many industrial research activities. IBM has used the Brookhaven synchrotron source for research on x-ray lithography, and although we seem to be losing our lead in this area to Japan, IBM/Brookhaven was certainly responsible for some of the first research on the subject.

In a somewhat different way, the Sandia National Laboratory has contributed extensively to radiation-hardened microelectronics and so-called III-V-compound semiconductor materials (gallium arsenide is the classic example in this class of compounds). Oak Ridge has done much work on processing technology and ion implantation. They have a first-rate ion/solid interaction laboratory, which they have used very successfully to interact with a variety of industries.

The other national laboratories have also performed research of relevance to the semiconductor industry, but to a lesser extent than the three mentioned above. The interactions are increasing due to concerns about the industry.

In the last few years, several industry initiatives have been launched as well. Erich Bloch, the Director of NSF, was involved in creating the Semiconductor Research Corporation (SRC), which was the first attempt by the industry as a group to cooperate. The purpose of the SRC is to fund research related to semiconductor technology at universities. Currently, the SRC supports very explicit research in design sciences, manufacturing sciences, and microstructure sciences at 43 different participating universities. It encourages interactions between these universities and the participating companies through regular reviews and visits.

At about the same time, another approach was taken by Admiral Bobby Inman. With the participation of a somewhat different group of corporations, he organized Microelectronics Computer Technology Corporation (MCC) in Austin, Texas. MCC is a cooperative research venture among about 20 corporations, with a 1986 budget of approximately $65 million. It is quite different from the SRC. Its research takes place at a single location and spans five programmatic areas: software technology, artificial intelligence, packaging and interconnection technology, advanced computer

architectures, and, recently, superconductivity.

MCC is not the only joint research activity. The RCA Laboratory, now called the SRI David Sarnoff Laboratories, is becoming a similar organization that plans to perform research in a contract mode. Bell Communications Research, in some sense, is another example of a laboratory in which a group of companies—the Regional Bell Operating Companies in this case—are involved in corporate R&D performed at a central location.

These are all original approaches to performing advanced technology research. They have already taught us much with regard to success in these endeavors. Both Brookhaven and Cornell have demonstrated that a critical factor in such success is the existence of well-focused programs directed at developing specific science and technology. Such a focus not only gives the program well-defined goals around which to build the project and make the facility unique, but also delineates a measure for success.

Whenever cooperative R&D activity is pursued, either between the government and industry or within industry itself, the goals of the activity must be clearly defined so that there is no question about when it is successful. In addition, funds must be sufficient to assure that the activity will be at the cutting edge of the relevant technology. Only then will the project attract the desired industrial users.

The goals of cooperative activities must be attainable, and not only with respect to the available funding. To be truly attainable they must also be commensurate with the culture and organizational structure of the institution at which the activity is to be centered. For example, one of the great strengths of our universities is the independence of each faculty member to pursue his or her research in an independent, entrepreneurial fashion. Thus, for instance, the university campus is probably not the optimum environment for projects that involve a large group doing cooperative development work.

The national laboratories, on the other hand, are designed to manage large projects. As previously mentioned, these institutions have made important contributions to the research and early development of x-ray lithography and semiconductor devices utilizing III-V compounds. The national laboratories are best when large, specialized equipment is involved. But again, they cannot take on projects that do not fit into their culture.

Another important issue essential to the success of such facilities is technology transfer. While in management at Sandia National Laboratory, I frequently found that our biggest problem was that people in industry simply did not know about the things we were doing. To effect technology transfer, we had to make a considerable effort to introduce industrial representatives to our work. Once that barrier was overcome, exchange of technology, whether a glass seal for batteries or new radiation-hard photodetectors, was usually straightforward. Therefore, when cooperative efforts are established, some means to inform and actively involve industry must also be created.

From this discussion, it is clear that we already have considerable support of the semiconductor industry through our universities, national laboratories, and the several cooperative activities mentioned. Nonetheless, the recommendations of the Defense Science Board and the Semiconductor Industry Association have prompted the organization of a new cooperative activity directed at much nearer term development challenges confronting the industry.

These recommendations call for the formation of a new corporation, called SEMATECH, which would address two major issues. The first has already been mentioned: the diminishing market share of American firms in the area of large computer memory devices, such as dynamic and static random access memories (RAMs).

The second issue is the rather more desperate situation that U.S. manufacturers of semiconductor processing equipment find themselves in with respect to Japanese competition. A healthy U.S.

semiconductor industry requires good processing equipment that is available here *first*. SEMATECH is to be a manufacturing facility for high-volume, high-density VLSI integrated circuit chips that would provide an environment for testing new processing equipment. In this manner, it would address both of the above problems.

The SEMATECH proposal calls for a budget of $250 million per year, with half to come from industry sources and half from the federal government. Current plans show the laboratory located in Austin, Texas. From its description, the SEMATECH concept places much more emphasis on development than any previous government-industry coupling, except those directly tied to defense. It will be most interesting to see if the industry as a group can cooperate in accomplishing SEMATECH's goals.

While this brief summary shows that there is a wide range of new and not-so-new cooperative activities between the government, universities, and the semiconductor industry, much more can be done. The national laboratories can be made more effective by including industrial competitiveness as part of their mission and by greatly enhancing their coupling to the industry. Many barriers to such activity have been eliminated by recent legislation. But we can go further toward establishing collaborations in which proprietary information is honored, yet the resources of the universities and the laboratories are focused on the competitive issues. The next few years may well herald a new era in industrial cooperation.

Both Brookhaven and Cornell have demonstrated that a critical factor in such success is the existence of well-focused programs directed at developing specific science and technology.

DISCUSSION AND COMMENTARY

STUART EIZENSTAT: One of the most promising developments in this country in the last several years is a recognition, born of necessity, that we must fundamentally change the way our institutions interact, both within and between private industry, government, and universities.

I don't believe we can transpose the Japanese industrial policy model onto the United States'

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culture. But we can learn from their experience, utilizing that which is applicable to our own society.

Dr. Brinkman was, in essence, describing a process of moving from an adversarial to a more cooperative society. The structure of our whole society is highly adversarial: our system of government with its divided responsibility between the Executive and Legislative branches; our Congress, with its division between the House and the Senate; our labor-management relations; our legal system.

Society doesn't have to be structured that way, but that is largely how we have structured it in the West. I don't think we can go through a national metamorphosis overnight, but we can learn that an adversarial process carried too far can lead to disaster. I am an attorney, and I must say that the legal system is a prime example of a nonproductive adversarial process.

If we are going to solve some of the competitiveness problems in the high-tech and R&D areas, it will be because we are finally learning that the real adversaries—and I do not mean this in the sense of enemies, but of challengers—are our overseas competitors, who have learned to be more cooperative in their domestic relationships, and who are beating the socks off of us every day in the marketplace.

We are finally beginning to accept that we need to explore avenues for greater cooperation within our present structure. In the labor-management area, for example, those companies employing more profit-sharing programs, or adopting quality circle plans (that we, in effect, taught the Japanese, but never believed in ourselves) are the very companies that are now at the leading edge of productivity in our own country.

No doubt, the kind of cooperation that Dr. Brinkman talked about between government, the private sector, and universities is essential and is the most exciting development in the area of competitiveness over the last five years. Of course, it is an open question whether this will

succeed, since it is so contrary to our normal adversarial process. It will take a lot of compromise in our own mindsets to make these different cooperative arrangements work. But current necessity may well force this kind of cooperation to a successful result.

As usual, federal policy follows rather than leads the way. But there have been some positive signs. In 1986, when so many tax incentives for business were being cut back or eliminated, literally the only new tax credit added to the revised tax code was a university basic research credit (distinct from the corporate R&D credit mentioned earlier), which provides to high-tech U.S. firms an incentive to contract out their basic research to universities by giving them incremental tax benefits. I think this signals the beginning of a recognition by our federal policymakers that they have to encourage the kind of cooperation that the private sector has taken the lead in.

Q: How can we express in concrete terms the nation's intention to reach some particular goal—to assert our national will, so to speak? For instance, if we assert that the United States *will be* a major force in the semiconductor industry for the next 20 years, what can we do to realize this goal?

WILLIAM BRINKMAN: I think expressing the national will is really relatively easy. The federal government must try to make clear statements about the direction in which this country is supposed to go. It can do that by announcing that it will fund activities that point us in a certain definite direction. The success of the Japanese is due in some measure to such direction. The VLSI effort and similar consortia have clearly pointed the direction of the country. It takes leadership to do that, and that is what we need. I don't see it as a difficult thing. It is just that somebody has to understand the situation well enough at a high enough level to do it.

ERICH BLOCH: What vehicles do we have to voice our national will? First, we have public elections. Second, we have the federal budget itself. The budget is really the expression of a national policy. As was said earlier, those in the R&D community who see the problem clearly must make their point, and if they do that successfully they will become part of the budgetary process, and the results will show up in the budget. Let's not forget that, no matter how worthy our ideas are, the world still runs on dollars.

In regard to the leadership that Dr. Brinkman spoke of, I think it is very important to focus our efforts on specific industries. General kinds of government pronouncements usually don't contribute greatly. I think focusing on a particular industry or a particular situation is the thing to do, and that is why I supported the SEMATECH effort. It focuses on an industry that is very basic and is in great need. We should see what can be done together between industry, government, and the academic sector to make that industry survive, even though today it looks pretty bleak.

COMMENT by ALAN SCHRIESHEIM (Symposium panelist; Director, Argonne National Laboratory): I would like to comment on an aspect of the technology transfer issue that has been mentioned. Congress has, in a good-hearted way, passed a number of bills that enable the national laboratories to transfer technology on a proprietary basis—in effect, to give a company a proprietary lock, if you will. But I don't really think that industry quite understands what these laws mean in terms of a company's relationship to the national labs.

Complicating this is the fact that American industry is not monolithic. So the national laboratories are faced with the challenge of dealing with a spectrum of different user communities. They must deal with small businesses that have limited technology and financial resources, with large corporations, and with these newly formed research consortia. They also have the chance to deal with the financial community in the form of venture capitalists.

Nonetheless, it is marvelous that the government has now empowered the national labs, with

their enormous capabilities and resources, to deal across that interface and to transfer technology much more effectively. But it is not at all clear that there is wide knowledge of this.

Q: Much technology transfer to other countries has been blocked in the past on the basis of national security considerations. For instance, in the President's recent superconductor initiative, he has enunciated a policy to widen the exemptions of material subject to the Freedom of Information Act to include commercially valuable information gained in government laboratories. We have also heard much lately about the easy access foreign nationals have to our universities. Is there a danger that academic freedom might be compromised by the new policies designed to stem the flow of technology to our competitors?

ERICH BLOCH: The biggest mistake we could make is to compromise the openness of our university system. I don't really think you could successfully restrict access to it, but even if you could, it would be a mistake. What we should do, however, is insist that there be a quid pro quo—an equal exchange. There are many government and university laboratories, both in industrialized and developing countries, at which important work is going on. We should insist on the same kind of access to those laboratories that we give to foreign nationals here. If we can obtain this quid pro quo, I think both countries will benefit from the exchange of basic research information at an early point in time.

Q: What are the prospects of developing a national strategic plan for pursuing R&D in this country? Isn't this the kind of coherent leadership we require to assure our future competitiveness?

STUART EIZENSTAT: If you are talking about a strategic plan from the federal government, there are several major obstacles. Which agency of the federal government would draft it? How would it be implemented?

We have a system of government unlike a parliamentary system in which one could logically think of a strategic plan being drawn up and, through the aid of a prime minister who controls a majority in parliament, passed in some coherent fashion.

Our system of government is simply too divided to really permit that kind of strategic planning. There is no instrument in the Executive Office of the President responsible for strategic planning. Instead, we have a highly incremental system of government in which improvements are made on a step-by-step basis. This makes it very difficult for industry to get the kind of clear signal it needs from the federal government about the direction national policy will take.

In addition, policy is constantly changing. As Secretary of State George Shultz once said in relation to foreign policy, the problem in Washington is that an issue is never truly resolved. This is equally true of other policy areas as well. We simply have to work within that incremental system to make the kinds of improvements we seek in R&D funding, NSF funding, and so forth. But the development of a sort of "macro" strategic plan that then can be implemented as a nationwide blueprint is simply not going to happen in our system.

Q: But will the halting progress our "incremental system" allows be sufficient for our competitive survival?

STUART EIZENSTAT: We are talking about the United States of America, not a corporation. A corporation can sit down and develop a strategic plan for itself and decide the product mix and direction it wants to take, allocating its resources accordingly. But, no matter how wise the President or how interested the Congress, it is impossible to think that the federal government can, in some similar way, develop a strategic plan and then implement it coherently. That is not to suggest that we shouldn't keep educating the public and banging on our policymakers to recognize the importance of R&D. But the improvements are

going to come in marginal ways. Yet improvements *will* come. Let me give some examples. Over the years, technology transfer has been an issue. For instance, there has been a hue and cry about disincentives to doing research with the federal government. In response, a change in patent policy followed after several years.

Similarly, there was a call to reduce the antitrust barriers to cooperative research among companies. So, over about two and one-half Congresses, we finally got a bill passed and signed by President Reagan making some major improvements in antitrust laws. And we also finally got an R&D tax credit passed in 1981.

In other words, we do have to have a strategic concept of where we are going, and the private sector has to keep pushing policymakers to make changes within that broad framework. But, from my perspective of how the federal government and the White House work, you are not going to get any institution in our system of government to develop a coherent strategic plan for research, regardless of the administration.

We just have to keep biting at the margins and pushing the system. Ultimately, the system does respond, but it does so incrementally.

DAVID PACKARD: In closing, we should recognize that there are many positive steps being taken across the country in all of the areas discussed here. For instance, there is a lot of effort to improve the school system from the local level—to institute rewards for excellence in the various schools. The simple fact is that many people across the nation have recognized some of the problems we have spoken of. We have, perhaps, achieved the first step: to recognize that we have some problems and to define, within reasonable limits, what those problems are. I see much that is positive, and I think we have a very good chance of seeing some improvement in all these areas as we move ahead.

EPILOGUE

Polaroid Corporation, sponsor of the Symposium on Industrial R&D Strategies and U.S. Technological Leadership, marked its 50th anniversary in 1987. Polaroid's interest in fostering a dialogue on the challenges faced by the nation's research and development community stems from the philosophy of its founder, Edwin Land. Well-known as a tireless innovator, Dr. Land has inspired a generation of industry leaders committed to "inventing the future" through the skilled blend of imaginative science and corporate vision. Here are the recollections of two of them.

FROM ITS VERY INCEPTION, even before the development of instant photography, Polaroid has had an uncompromising commitment to research and development and the manufacture of good products. It has maintained an insatiable curiosity about things both practical and esoteric.

What struck me most when I joined the company so long ago, and during the 19 years I spent with it, was its commitment to conception, innovation, and invention. It was a fraternity of people genuinely excited by the notion of doing something that had never been done before—a group of people who translated concepts into useful inventions. This environment continues to the present.

Whether we were extraordinary people or simply ordinary people doing extraordinary things I am not altogether certain. I am certain, however, that there were among us some very special people—people like Edwin Land. He started it all. He had the best feeling for invention, for innovation, for science, for technology, for development, and for bringing a product to its potential users.

Once, during my first few years at Polaroid, I questioned whether a proposed product would sell. Dr. Land turned to me and said, "Elkan, we don't do market research. We create our markets with our products." And this has been his credo over the years.

Today, Polaroid continues to take pride in making products of its own invention. Over the years, it has, like many other great American companies, invented its own future several times over. Its history, like the history of so many companies involved in this symposium, is a testimony to the importance of the private research laboratory to U.S. technological leadership and its economic competitiveness.

Elkan Blout
Dean of Academic Affairs
Harvard School of Public Health
Treasurer, National Academy of Sciences

I HAVE HAD THE GOOD FORTUNE to have spent about 50 years with the Polaroid Corporation, from its early years as a small research company to today, when it is one of the Fortune 500. Many of those years were spent working for one of the most creative people of our time, Edwin Land. He was an inspiring, stimulating, and demanding person to work with. Land generated an environment that encouraged and utilized the creative capabilities of each of us. Nothing seemed too difficult. Almost nothing seemed impossible.

Polaroid is very different today from what it was in those early years. It is richer and has far more resources. But it also has more bureaucracy and tighter controls. It is a real challenge to any large industrial corporation to maintain the kind of stimulating, challenging, and intellectually rewarding environment that just seemed natural around Land. We really have to work at it.

George Bernard Shaw wrote something that reminds me very much of Land's research attitude. "You see things and say, 'Why?' but I dream things that never were, and I say, 'Why not?'"

William J. McCune, Jr.
Chairman of the Board
Polaroid Corporation

. . . . Americans bring instinctive Optimism, generosity, and enthusiasm to the support of any effort that they believe is fundamentally creative

FOR ABOUT 20 YEARS I have been interested in the question of how to establish in the United States a large number—perhaps several thousand—of new complains based in science. My dream was that each of these companies would conceive of a new field and would carry on from the basic scientific work in that field through research, development, engineering, production, aesthetic design, lively, honest advertising, and efficient distribution. . .

The specifications I now set for these ideal scientific companies are no different from the ones I set myself in 1927: pick problems that are important and nearly imposible to solve, pick problems that are the result of sensing deep and possibly unarticulated human needs, pick problems that will draw on the diversity of human knowledge for their solution, and where that knowledge is inadequate, fill the gaps with basic scientific exploration—involve all the members of the organization in the sense of adventure and accomplishment, so that a large part of life's rewards would come from this involvement. . . I have found that Americans bring instinctive optimism, generosity, and enthusiasm to the suport of any effort that they believe is fundamentally creative.

Edwin H. Land

From the Charles F. Kettering Award Address, "On Some Conditions for Scientific Profundity in Industrial Research," June 17, 1965